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PROBLEMS
IN THE
PSYCHOLOGY OF READING

BY

J. O. QUANTZ, PH. D.

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TABLE OF CONTENTS.

§ I. Introductory 1
§ II. Visual Perception.. 2
§ III. Sensory Types 18
§ IV. The Motor Type and Lip Movement 32
§ V. Other Determining Factors of Rapidity of Reading....... 41
§ VI. The Relation of Eye to Voice in Reading Aloud........... 46
§ VII. Résumé 49
Appendix:—Personal Sheet.. 51

PROBLEMS IN THE PSYCHOLOGY OF READING.

§ I. INTRODUCTORY.

In an age when the thinker writes his best thoughts and the investigator records his most important discoveries, when almost the sum-total of human knowledge is preserved in the form of books, and he whose ambition is scholarship spends his days in the alcoves of some great library, it needs no argument to show that a large part of the culture of modern life depends upon this source of information. If in these days 'the true university is a collection of books,' and that university is extending indefinitely, it is clearly important that we should understand the art of gaining the most from an author's words with the least expenditure of time and energy. We have books and articles innumerable telling us, from both the literary and the pedagogical point of view, how to read. But the psychical processes involved in reading have not been experimentally investigated. On the physiological side much has been done[1]; but psychologically, almost nothing. The present research[2] is an attempt to consider some of the problems which are presented in the processes of ordinary reading: for example, What are the factors which make a rapid reader? Does rapidity depend on mental capacity, alertness of mind, quickness of visual perception, amount of practice—on any or all of these? What sensory type of persons obtains and retains most—those who gain their knowledge principally through the eye, or through the ear?

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[1] Some of the recent articles are: Cattell, *Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben*, *Philos. Stud.*, II., p. 635, *Ueber die Trägheit der Netzhaut und des Sehcentrums*, *id.*, III., p. 94; Sanford, *Relative Legibility of Small Letters*, *Amer. Jour. Psych.*, I., p. 402; Griffing, *Visual Perception and Attention*, *Amer. Jour. Psych.* VII., p. 227; Griffing and Franz, *On Conditions of Fatigue in Reading*, *Psych. Rev.*, III., p. 513.

[2] A study from the psychological laboratory of the University of Wisconsin, undertaken at the suggestion of Professor Jastrow, and pursued under his sympathetic direction.

Rate of reading is the main problem, and an endeavor has been made to gain an initial understanding of some of its factors. Visual perception is first studied, with color, form and words (§ II.). Eye and ear are then compared as avenues of knowledge (§ III.), and with these another form of mental tendency is discussed; namely, motor-mindedness (§ IV.). In this connection lip-movement in silent reading is considered. Other sensory and intellectual factors are investigated as possible influences in reading (§ V.). All these are correlated graphically with reading rates, with a view to the discovery of their interrelations.

§ II. Visual Perception.

1. *Method.*—All normal reading involves visual perception. A series of tests was therefore made to determine the quickness of the perception of words, both isolated and in construction, and, by way of comparison, the perception also of form and of color.

The forms used were the circle, square, diamond, vertical and horizontal oblong, hexagon and crescent. These, in gray on a white background, were arranged in two lines on a card, which was placed before the circular opening of an exposure apparatus. On each card were twelve forms, in chance order, presenting an appearance as in figure 1, which shows the exact size.

The subject was required to name *aloud, in order,* and *as rapidly as possible,* these geometrical forms, as many as he was able to see while the card was exposed to view for a definite length of time (½″ and 1″). The card was shown repeatedly, until all the forms were read, the subject beginning to read at each exposure where he ended in the preceding.

For colors the method was similar. Colored discs of red, orange, yellow, green, blue, brown, black and white, of 14 mm. diameter, were shown on a gray card, the order as before being determined by chance. So with isolated words, those employed being words in common use, usually of but one syllable. For continuous reading, selections of 15 and 30 words in length were taken from a child's first reader ('great primmer' type).

2. *Apparatus.*—Great difficulty was found in devising an exposure apparatus which would give a sufficient range in time lengths, and yet be reasonably exact. The one used was devised by Professor Jastrow, and gives durations which are uniform within 0.02″, as measured by the Hipp chronoscope. The mechanical principle of it is simple and will be understood from the cut.

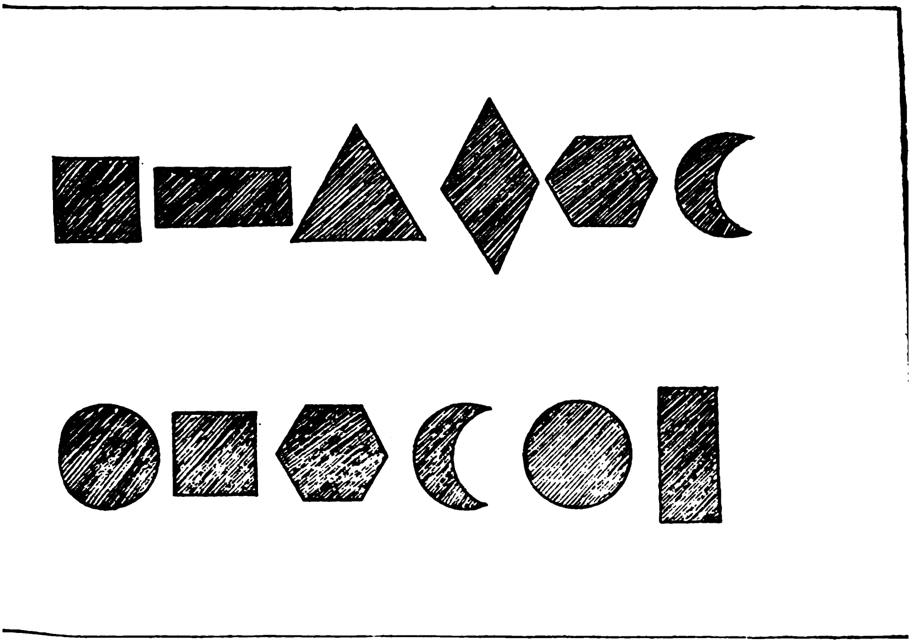


FIG. 1.

To a frame *fr* are attached two shutters, *s*, *s′*, which revolve about a centre pivot *c*. Around this a cord is coiled from which a weight *w* is suspended so as to swing free of the frame. This weight causes the shutters to revolve in the direction indicated, as soon as *s* is released from the catch *d*. *s* is the opening shutter, *s′* the closing one. When the catch is drawn back, the two shutters move around at the same rate (being clamped together at *m*) until *s′* reaches the position from which *s* started, the card being exposed to view during this time. The shutters move at an almost uniform rate of speed, as the short ends are weighted so that each shutter is in equilibrium in any position when *w* is removed. *p* is a small pin in one of the shutters, beneath which the cord passes—a

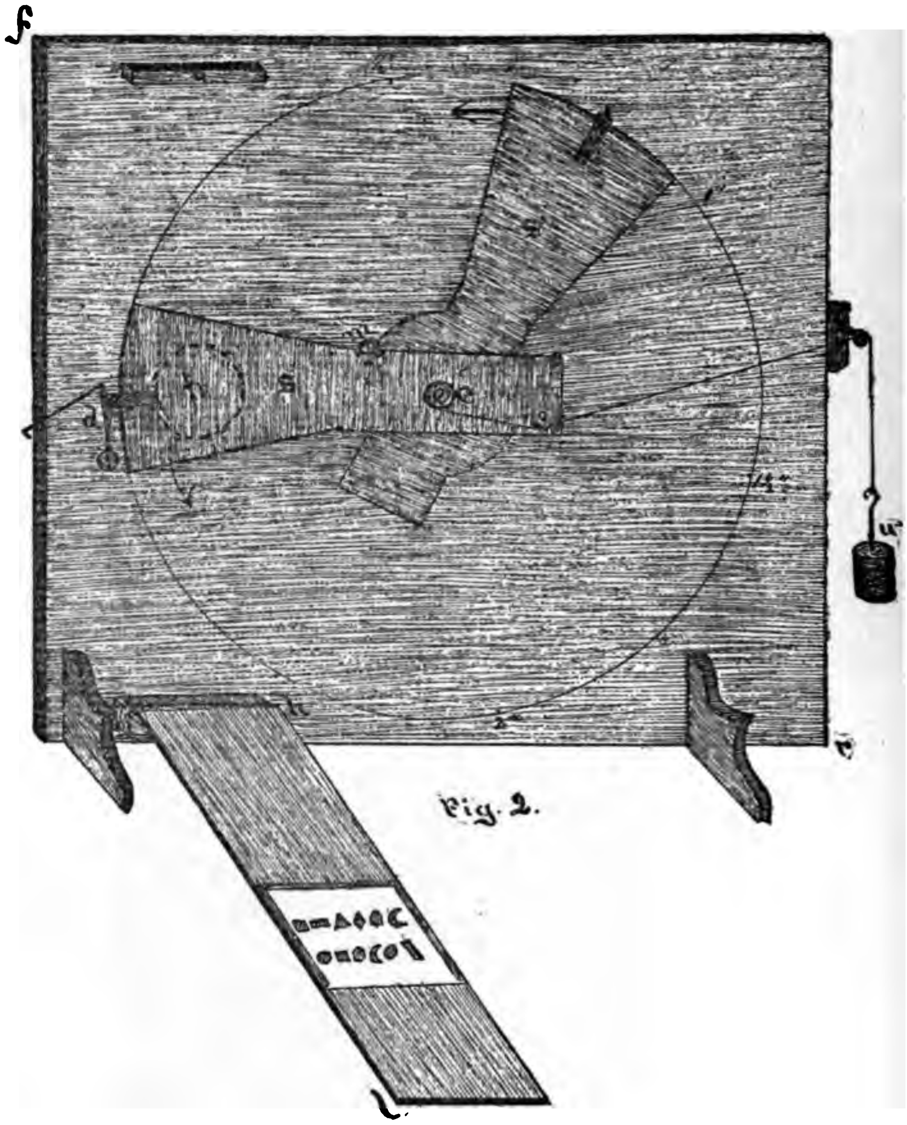


Fig. 2.

simple device for overcoming inertia in starting. The card is placed in a holder *hl*, hinged at the bottom, to allow the cards to be exchanged easily and also removed from before the opening while the shutters are being readjusted. In the cut the holder is shown let down; when brought into position it places the card directly before the circular opening, which is 136 mm. in diameter. The subject is seated at a convenient reading distance in front of the apparatus, the rear view of which is shown by the cut, the mechanism being thus concealed from him by the upright frame. His view of the card is cut off by the shutters except during the time *s′* requires to pass to the original position of *s*. This time is proportional to the angle which the shutters make with each other. Consequently different exposure lengths are obtained by changing the relative positions of *s* and *s′*, regulated by the screw at *c*; that is, while *s* remains in the position in which it is held by the catch *d*, *s′* is switched around and set at ½″ when a half second exposure is required, and so on for the other lengths. These graduations were originally determined with the chronoscope.

The observer accommodates his eye for the distance of the shutter before it opens, and the card is so close behind this that when seen it requires no re-adjustment of the lens nor change in convergence.

3. *Results and Interpretations.*—The results of the visual perception tests, with fifty university students, juniors and seniors, are given in Table I., which shows not only the relative legibility of forms, colors, words and sentences, but also the relative advantage, in point of speed, of different exposure times. The figures are given on the basis of the number of forms, colors, etc., read per second, and are thus comparable as they stand.

TABLE I.

LENGTH OF EXPOSURE.	FORM.	COLOR.	WORD.	SENTENCE.	AVERAGE.
0.5″	5.5	5.7	7.2	9.9	7.1
1″	2.8	3.5	5.2	8.8	5.1
2″				6.8	
Average	4.2	4.6	6.2	9.4[1]	6.1

[1] The 2″ test (6.8 words per second) is omitted from this average so as to make the sentences directly comparable with forms, colors and isolated words.

Half a second is seen to be relatively much more advantageous than one second (as 7.1 to 5.1, or a gain of 39%), and one second than two seconds (29%).

The shorter time is relatively more favorable because of (*a*) positive after-images, (*b*) primary memory images, and (*c*) less overlapping of mental processes.

(*a*) The positive after-image of the objects seen last persists for a short time after the closing of the shutter, and for the same time *absolutely* no matter what the exposure length may be. Consequently it increases the time *relatively* more in the shorter exposures than in the longer.

(*b*) The memory after-image, sometimes pretty clearly defined, seems to flit before the eye, after the disappearance of the object itself, only waning after several seconds. This amounts practically to a lengthening of the observation time, though the vividness of the object is of course diminished. The gain thus introduced would be relatively greater for the shorter exposures, as all the objects actually seen could be held in the image-complex, but not so with the longer exposures.

(*c*) The shorter exposure-times have an advantage, moreover, in that the different processes do not retard one another by overlapping so much as when the times are longer. The impression made on the retina requires some time to reach the sight center, and become a sensation. But the sensation may be interpreted, that is, may become a perception, after further stimuli are cut off by the closing of the exposure apparatus. The association between object and name may also be formed later, and the motor processes involved in naming be carried out. Thus the various physiological and mental processes are not required to overlap to the same extent in short exposures as in longer, where the form, color, etc., must be distinguished and named while additional sensations are being received. The shorter exposures really include, therefore, for the interpretation and naming of the sensations, a certain length of unreckoned time in addition to the actual exposure-time. The distinction- and choice-time required for discriminating and naming geometrical forms, words, and colors, is certainly not insignificant. The reaction-time for short English words, according to Cattell, is 0.409″, and for colors (in a group of ten) 0.601″. Deducting 0.170″ as the simple reaction-time for sight, we have left, for the purely mental processes of distinction and choice, 0.239″ for words, and 0.431″ for colors. For geometrical figures this fraction would be larger, as these are read more slowly than either colors or words. The fact that they are so read, and that the difference between the 0.5″ and 1″ rate is much larger than in colors or words, goes to show that the last factor named, the overlapping of processes, is important.

The supposition that these factors do enter is further strengthened by the fact that continuous silent reading at the highest rate of speed at which one is able to read intelligently, under ordinary conditions, is much less rapid than under the special conditions of this experiment. The average maximum rate of all students was 6 words per second in continuous reading, as compared with 8.5, the average of the 0.5″, 1″ and 2″ exposures.

Turning again to the foregoing table we find that colors are perceived and named more rapidly than simple geometrical forms (as 4.6 to 4.2, or an advantage of nearly 10%); isolated words more rapidly than colors (6.2 to 4.6, or a gain of 35%); and words in construction than words detached (9.4 to 6.2, or a gain of over one-half). The relative perceptibility of these classes of things can best be shown by a comparison curve.

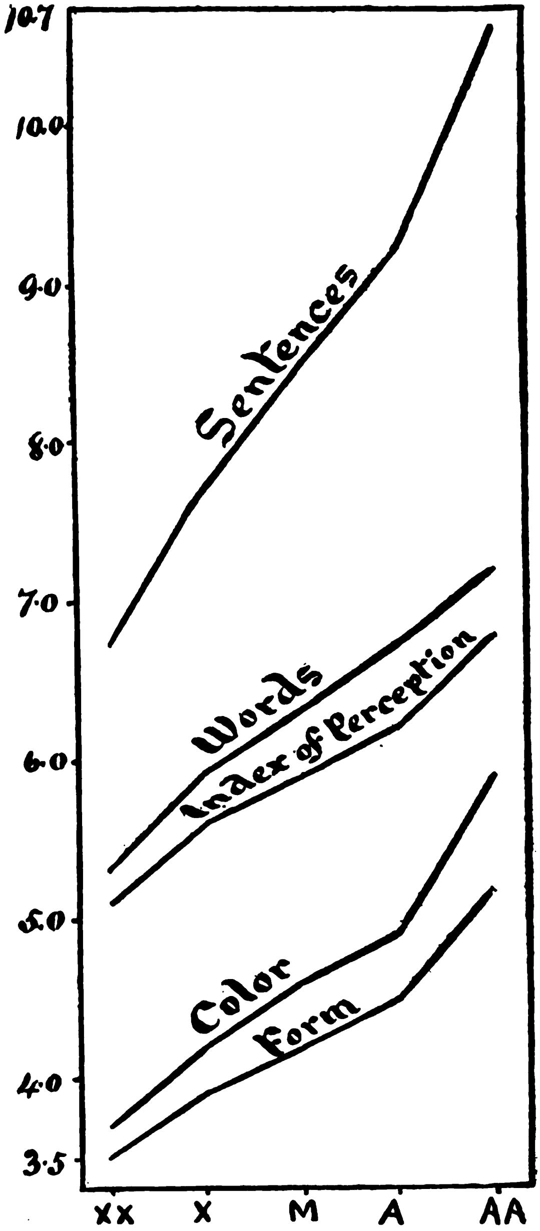
The numerals at the side indicate the number read per second. The letters below designate the classes, *XX* being very slow, *X* slow, *M* medium, *A* rapid, and *AA* very rapid. The groups contain approximately equal numbers of individuals; but they are not the same individuals throughout, that is, the *M*'s, for example, of form are not in all cases the same persons as the *M*'s of words, since those individuals who perceive forms with medium quickness may fall into a different class in regard to reading words.

Curve 1 shows:

(1) That forms, colors, isolated words and words in construction are in ascending order in the rapidity with which they are perceived.

(2) That there is less difference between the rates of form and color (the curves lying closer together) than between the others.

Curve I.


10.7
10.0
9.0
8.0
7.0
6.0
5.0
4.0
3.5
Sentences
Words
Index of Perception
Color
Form
XX
X
M
A
AA

(3) That there is less difference between the slowest (*XX*) class in the different types of perception than between the most rapid (*AA*), except in the relation of color and words (the lower ends of the lines are not so widely separated as are the upper ends.) Those who are poor perceivers show a narrower range between the different types of perception than do those who are good perceivers; that is, for them the different kinds of perception are less differentiated. Consequently

(4) That there is more variation in rapidity between the most rapid and the slowest in sentence reading than in the other tests, and slightly more in color than in form or in words (the sentence curve is the longest).

(5) There is more difference between the 'good' and the 'very good' than between any other two groups (the part of the curves connecting *A* and *AA* tends upward more than any other part). This is the common observation that those who are exceptional in anything are farther removed from the merely 'good' than these latter are from the average.

(6) The curves are very similar, which means that many of the same factors enter into the perception of forms, words and sentences. That the curves have the same general tendency but are not absolutely similar shows that these influencing factors enter in varying degrees into the perception of the different sorts of objects.

The 'index of perception' is formed from averages of all the types of experiment (the average being formed not from groups, but from individuals, thus making it more exact), and therefore lies between the other curves. It represents the general perceptive power of the different groups.

The large difference in the rate of perception in the different types is due mainly to association. The association between the written and spoken word is much better established because much more frequently formed than is that between the geometrical figure and its name. It is moreover more definite. In adult life we 'intuit' a word as a whole (at least in the case of short familiar words, such as were used throughout these tests), but the combination of characters forming it has a definite meaning; whereas the forms may be designated by different names.

For example, ◇ is called a diamond or a rhombus, ○ circle or globe, ☾ moon or crescent. This is returning to the indefiniteness of primitive picture-writing as compared with the highly evolved phonographic language.

The association is of the same sort in words as in forms or colors, for the connection between the written symbols and the spoken sound of any given word is just as arbitrary as is that between a particular geometrical form and its name as uttered. But the association between forms or colors and their names, being less necessary than between written and printed words, has been less frequently formed and the former has remained a voluntary process while the latter has become automatic through repetition. That the strength of an association is measured by the time it requires is abundantly illustrated by experiments in complex reaction-times; for example, the times required to name a picture in the vernacular and in a tolerably familiar foreign language were 0.477″ and 0.649″ respectively.[1]

The same explanation applies to colors: the association between color and name is closer than that between form and name, but not so well established as between written and spoken word (0.409″ for words, 0.601″ for colors, are the reaction-times, as already given). In the continuous reading, moreover, the thought introduces a thread of connection between the succeeding words, and thus a readjustment of attention in passing from one word to another is not required. The reaction-time for words in construction is 0.138″, which is much less than for disconnected words.

No. 2 is a distribution curve. It is formed, therefore, not by putting an equal number of individuals into each group, but by making equal absolute differences between the number of words, etc., read per second by the different groups—that is, the range in perceptive power is the same for each group.

Curve 2 shows:

(1) That in visual perception, as in the distribution of individuals according to any other mental or physical test, many cluster around the average point, falling off gradually in both directions measured by equal absolute differences, there being

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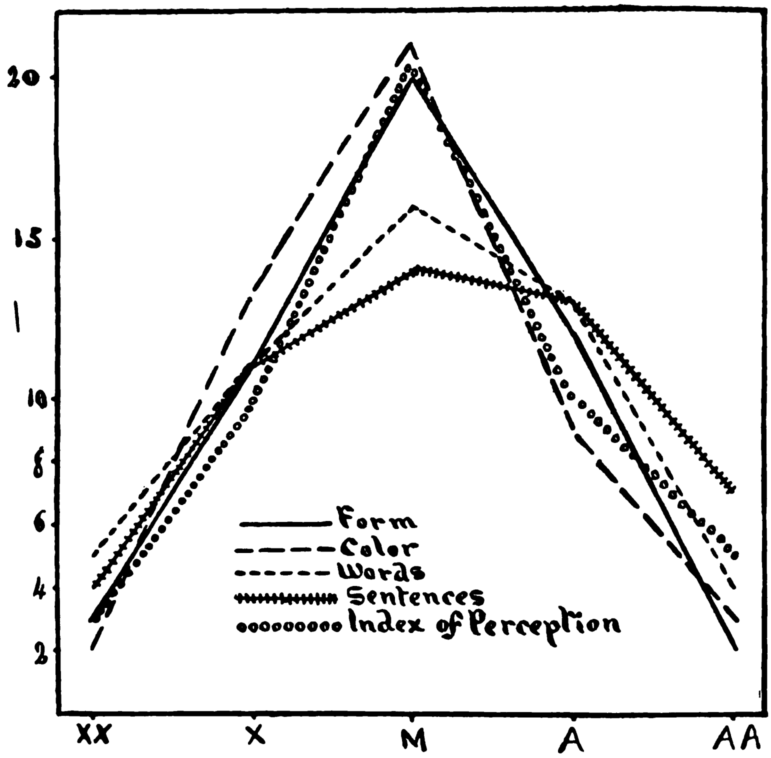
[1] Jastrow: *Time Relations of Mental Phenomena*, which see for examples illustrating this principle.

relatively few who are either exceptionally good or humiliatingly poor. The probability is high that an untried specimen will be found to be near the average.

(2) That the number of persons represented in any class decreases at about the same rate above and below the average. (The curves are tolerably symmetrical.)

Curve 2



(3) That in the reading of isolated words and of sentences the number of persons who are near the average is comparatively fewer (apices of curves are not so high); that is, individuals are relatively more like one another as regards their capacity to read forms and colors—the less familiar processes.

4. *Correlations.*—In addition to comparison and distribution curves we shall have occasion to use what are known as 'correlation curves,' and a brief explanation of these may be

given. If one thing completely determines another, or, in mathematical phrase, if one is a function of another (for example, if quickness or slowness of visual perception is the sole determining factor of increase or decrease in the rate of one's reading), or if two things depend upon identical conditions (for instance, if a subject's maximum rate of intelligent reading is determined by absolutely the same factors as his normal rate), then in both cases the second of these will increase proportionately to the first.[1] The graphic method of illustrating this by correlation curves is somewhat as follows[2]: Dividing all the persons tested into groups, say 5, with an equal number of individuals in each group,[3] according to their *maximum* speed of reading, and naming these classes, as before, *XX* (very slow), *X* (slow), *M* (medium), *A* (rapid), *AA* (very rapid), we find the *normal* rate of reading of each of these classes. This gives the following table expressed in number of words per second.

TABLE II.

Classes	XX	X	M	A	AA
Maximum Rate	4.3	5.2	5.7	6.5	8.7
Normal Rate	3.8	4.4	4.6	5.6	7.0

That is, when the principle of division into classes is according to maximum speed of reading, those who read at their *highest* speed 4.3 words per second, and are therefore in the lowest class, are able to read at their *ordinary* rate 3.8 words per second. And similarly for the other classes.

Now, if we take a rectangular surface divided into small squares, and graduate one side of it (horizontal) with numbers nting *normal* reading rates, and an adjacent side (ver- with numbers which correspond to *maximum* reading ve can find a point in this rectangle whose horizontal n is represented by 3.8 and vertical by 4.3—the numbers ating respectively the normal and maximum rates of eaders who are 'very slow' when classed according to naximum rates. Similarly points may be found for each

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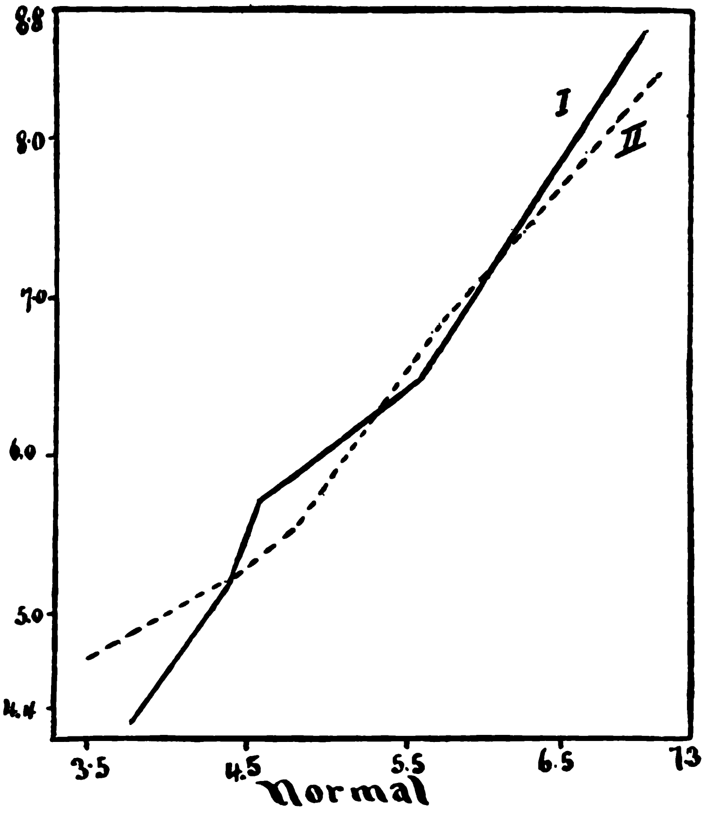
[1] It will perhaps be said that Weber's law contradicts this; external stimulus is the cause of sensation, but a vibration of air of intensity $2x$ will not cause twice as loud a sound as a vibration of intensity x. But it is to be remembered that external stimulus is not the sole cause of sensation. Physiological conditions must be included.

[2] See article by Dr. Franz Boas on Correlation in *American Anthropologist*, July, 1894.

[3] Equal *differences* in rate between the groups, or any equal *range* of variation in each might be chosen as the standard of division, instead of equal *numbers* in each group; but the latter method is employed throughout this research as being more suitable where the whole number of individuals is not large.

Curve 3.



other classes. Connecting these points gives a line (I. in 3) which we may designate the 'curve of normal reading determined by maximum rate.'

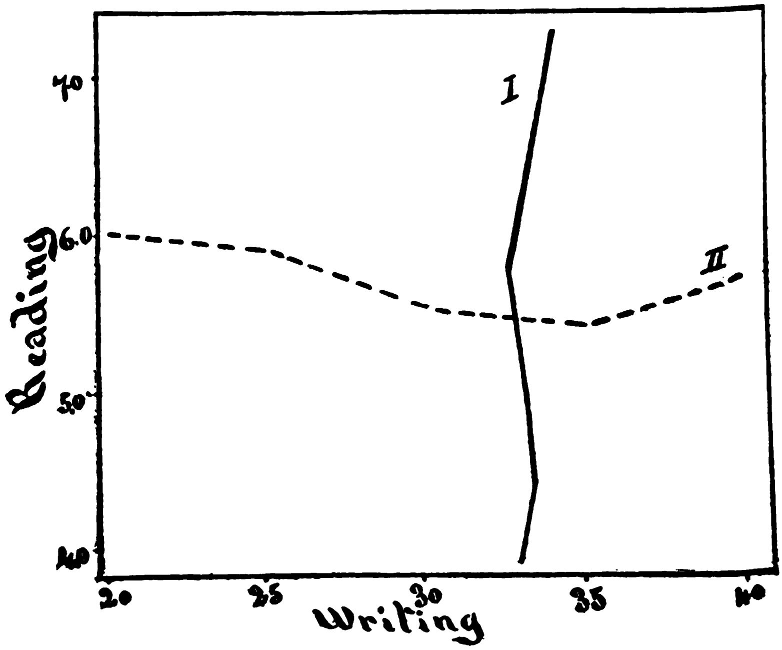
rther, we may group these individuals into classes deter- by their *normal* reading rates, and find the average maximum rate of these classes, thus obtaining the following table, which gives a second curve, namely, 'maximum reading rate determined by normal.'

TABLE III.

Class	XX	X	M	A	AA
Normal rate	3.5	4.3	4.8	5.8	7.1
Maximum rate	4.7	5.1	5.5	6.8	8.4

This completes the correlation and shows at a glance how closely related maximum and normal reading are; that is, how nearly the same forces determine both. If the two phenomena

Curve 4.



were determined throughout by the same factors, and to the same extent, the curves would exactly coincide on the diagonal of the rectangle. If the influences operative in both were largely, but not entirely, the same the curves would have the same general direction, but would cross near the middle and diverge slightly toward the ends (see Curve 5). The fewer the common factors the wider would be the divergence (cf. Curve 4 with 5), until when the two progressions under consideration were absolutely independent of each other the 'curves' representing them would be straight lines crossing at right angles—provided always the number of records included in the calculation was sufficiently large to eliminate the influence of accidental variations. For example, if we were to correlate speed of mechanical writing with reading rate and find the classes to be as follows:

TABLE IV.

WRITING DETERMINED BY READING (I.).

Classes	XX	X	M	A	AA
Reading	3·9	4·4	4·9	5.8	7·3
Writing	33	33·5	33·2	32.5	34

READING DETERMINED BY WRITING (II.).

Writing	20	25	30	35	40
Reading	6.0	5·9	5·5	5·4	5·7

It would mean that the rates of reading and of writing were practically independent of each other, as shown by curve 4.

As the rate of reading advances through the several classes from slowest to most rapid, the speed of writing, being uninfluenced by that of reading, remains with almost the same average in each class throughout. Thus the curve of 'writing determined by reading' (I.) is vertical, not tending far to right or left. Similarly the curve of 'reading determined by writing' (II.), where the individuals are grouped according to rates of writing, advances horizontally, not deviating to any great extent either up or down, since the different rates of writing all correspond to the same reading rate. This, then, shows that reading and writing rates are almost independent of each other.

Further, if some of the influences which produced greater speed in writing were actual hindrances to rapidity of reading, and *vice versa*, the curves would revolve still farther in opposite directions; that is, they would again approach each other, but in this case the 'upper ends' of the curves, representing the highest rates, would lie toward opposite ends of the diagonal of the rectangle (see Curve 11). This approach would continue as the determining elements became more antagonistic, until, when the factors which produced speed in the one process were exactly the same which caused slowness in the other,—that is, when the determining influences were entirely incompatible with one another,—the curves would again coincide, the beginning of one falling upon the end of the other, and *vice versa*. Through this general explanation the correlations which follow will be easily understood.

The figures connecting visual perception and reading rate[1] are as follows:

TABLE V.

VISUAL PERCEPTION DETERMINED BY READING RATE (I.).

Classes	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Perception	5.35	5.7	6.05	6.3	6.3

READING RATE BY VISUAL PERCEPTION.

Perception	5.06	5.64	5.92	6.17	6.77
Rate	4.22	4.59	5.31	5.42	6.62

By this method of representation, therefore, the relation of visual perception to reading rate is shown by curve 5.

This shows that mere quickness of perception, with its large physiological element, is an important factor in deciding one's rate of reading, as the lines of curve 5 are far from being perpendicular to each other. That they are also far from coinciding leaves room for other large factors in the determination of speed of reading, as the sequel will show.

The number and the nature of the errors made in the visual perception tests deserve, perhaps, a word of comment. Out of a possible 48 the average number of errors was, for form 1.4, color 2.7, words 0.5, sentences 0.6. Inversions of order and omissions are common throughout; but substitutions, of course, occur with far greater frequency. Hexagons are often mistaken for circles, these two figures resembling each other more closely than any of the others used in the tests. Most errors are made, as one might expect, in the naming of

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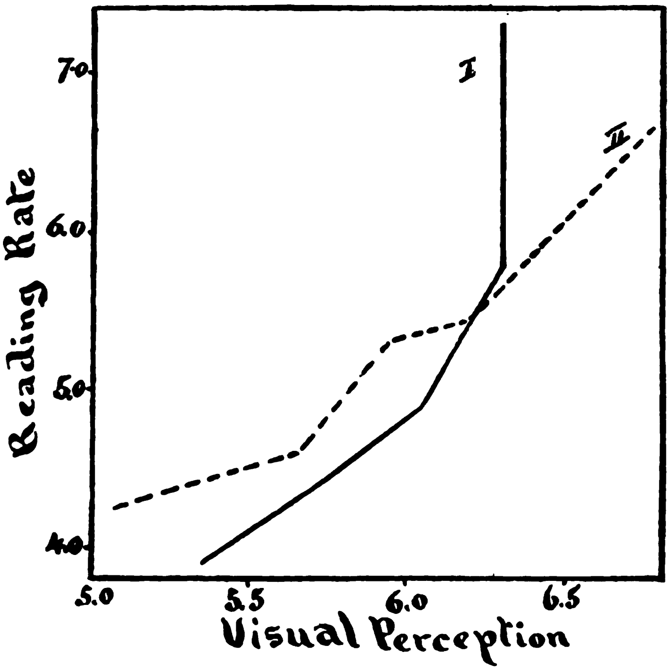
[1] The index of 'visual perception' was obtained by taking the average of forms, colors, words in isolation and in construction. 'Reading rate' here, and as used throughout, unless otherwise stated, is the average of the maximum rate under ordinary conditions and the normal rate of both the long and the short selections of silent reading. Reading aloud is not included as it does not show individual differences as silent reading does; *e. g.*, in silent reading the variation is all the way from 3.3 to 12.2 words per second for maximum rates, and 3.5 to 8.8 for normal; while in reading aloud the differences are within a range of 2.6 to 3.9 for normal rates.

Curve 5.



colors. This may, to some extent, be accounted for on the supposition that some subjects, especially men, misname a color even when seeing it correctly. An effort was made to eliminate this source of error. Whenever it was suspected the subject was tested by means of color cards not used in the perception experiments. Before the color experiments were made, moreover, the subject was shown all the colors that he would be required to name, and his color designations corrected when in error. Notwithstanding these precautions errors were comparatively frequent. Orange was mistaken for yellow 29 times (by all subjects in the whole series of experiments), and yellow for orange, twice; orange is called red 20 times and red orange four times; blue is called green four times, and green blue ten; red is said to be brown 16 times, and brown red, twice. When seen by a momentary glance, therefore, colors are very liable to be mistaken for one another, and the uniformity of this error seems to follow the rule that adjacent colors of the solar spectrum are most likely to be confused—orange with red or yellow, green with blue, etc.

The errors in the perception of words were less numerous. In reading isolated words the substitutions were of those similar in appearance, while in sentence reading the substitutions were of words similar in sense. These errors are not due to mere guessing; the subject 'sees' what he reads, as has been noted by other observers.

§ III. SENSORY TYPES.

1. *Methods of comparison between eye and ear.*—Dismissing for the present the tests in which the eye alone was employed as the receptive organ, we may proceed to a class of experiments in which eye and ear are both involved, and institute a comparison between these in their powers of acquisition and retention. Considering language only, is the eye or the ear a better medium for the gaining of knowledge? It is a pedagogical question of no little importance. In view of what has already been written on the subject[1] we may at once answer: Some persons are ear-minded—they think most readily in auditory ('phonographic') images; others are eye-minded, thinking in visual ('photographic') images;[2] while a probably much larger number are fairly well balanced, using either process indifferently, or both together. Our present interest being only in the relative advantage of the visual or the aural bias, our specific question becomes: Are persons who are distinctively of the visual type more rapid or more intelligent readers than those of the auditory type, or less so?

The comparison between eye and ear was made in three ways: (1) by testing the visual and auditory span; that is, the limit of power to repeat correctly words read or heard once; (2) by detection of differences between two variant readings of the same passage; (3) by the ability to reproduce the thoughts of two selections, one of which was read to the subject, the other read silently by him *at the same time.*

2. *Memory Span Test.*—In testing the 'span of prehension' (as it has been called[3] from its analogy to apprehension and comprehension, to both of which it is related as a simpler process), short one-syllabled substantives were used, of four, five

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[1] Jastrow: *Eye-mindedness and ear-mindedness*, Pop. Sci. Mo., Vol. XXXIII., p. 597, from which the following methods are borrowed in substance; Egger, *La Parole intérieure*; Galton, *Inquiries into Human Faculty*, etc.

[2] "Certaines personnes sont plus sensibles à l'excitation esthétique ou intellectuelle quand elle se présente sous la forme d'un tableau et passe par les yeux, d'autres quand elle s'incarne dans une symphonie et arrive à l'esprit par les oreilles. Ce sont ces excitations-là qu' elles rechercheront. Les créateurs se serviront aussi de ces formes concrètes pour exprimer, pour dépenser leurs propres émotions." Paulhan, *Les Caractères*, p. 123.

[3] Joseph Jacobs, *Mind*, XII., p. 75.

or six letters, as being less confusing and less variable in their relative difficulty than nonsense-syllables, and more closely allied to the sort of memory we continually exercise.[1]

It is doubtless true that eye- and ear-mindedness should be subdivided. One person, for example, remembers forms most easily, another colors; memory is to be resolved into memories; yet the general types, visual and auditory, are sufficiently well marked.

The tests on which the comparison is based were of three kinds:

(*a*) Auditory, in which the words were read *aloud to the subject*, at a rate deemed the most favorable, about one hundred per minute.

(*b*) Visual, in which the printed words were read *silently by the subject*, at the same rate as before.

(*c*) Visual and auditory combined, in which the lists of words were read *aloud by the subject*, thus giving him the advantage of both eye and ear as receptive organs.

In each case beginning with four, the number of words read to the subject or by him was gradually increased to the limit of his capacity for repeating the words with absolute correctness—the correct words and in the right order—the words being reproduced without any interval after the reading of each list. Rhythmic grouping of words in reading was avoided. A marked tendency to group into 3's had especially to be guarded against.[2] Ebbinghaus[3] found that memory span was considerably greater when the material to be repeated was grouped than when given at a uniform rate throughout. There is a possible analogy between successive rhythmic grouping of words read and simultaneous visual grouping which increases the extensive limit of intuition.

The average of all subjects was: Auditory span of prehension, 5.7 words; visual, 5.5; auditory and visual combined,

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[1] The kind of material used, however, was not important, as our interest was not in the *absolute span*, but in the *relative span* between eye and ear.

[2] For the rhythmic tendency in general see T. L. Bolton, *Rhythm*, *Amer. Jour. Psych.*, Vol. VI., No. 2. He finds that 3 is an unfavorable group; but his 'clicks' were much more rapid than the reading of words. Consequently a larger number in the group would be more common.

[3] *Das Gedächtniss.*

6.3. The number of letters repeated is slightly larger, and numerals exceed these considerably, probably because our attention has to search among only 9 numerals but 25 letters (omitting 'double w'), and because we are accustomed 'to take letters in groups having a phonetic value, but numerals have few if any associations of contiguity; we find them in haphazard order."[1]

More difference was to be expected than is found between the single process of eye or ear alone, and the double process of eye and ear reinforcing each other. Those who are decidedly auditory or visual are not able to do much more creditably by the help of both eye and ear than by their favorite single process of either vision or audition. Actual results show indeed that so far from the addition of a second sensory process being any advantage for individuals who are distinctively of one type, it is in many cases an actual hindrance. This would mean that in the case of decided 'visionaires,' or 'auditaires' the simultaneous use of a second sensory avenue interferes with the concentration of attention upon the sensations received through the primary.

Correlated according to this memory-span test the eye-minded student is a more rapid reader than the ear-minded, though the difference is not striking. The numbers of Table VI express the visual-auditory ratio in percentages; that is, they show what per cent. the visual memory-span is of the auditory. The *XX*'s therefore of 'eye-mindedness' include those subjects who are decidedly of the auditory type, as distinguished from the visual; the *M*'s, those who are balanced between eye and ear; and the *AA*'s, those who are strongly visual.

TABLE VI.

MEMORY SPAN DETERMINED BY READING RATE (I.).

Classes	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Span	94.99	93.56	96.2	98.66	101.8

READING RATE BY MEMORY SPAN (II.).

Span	76.8	83.5	100.0	120.5
Rate	4.68	5.16	5.46	5.56

From this the following curve is constructed:

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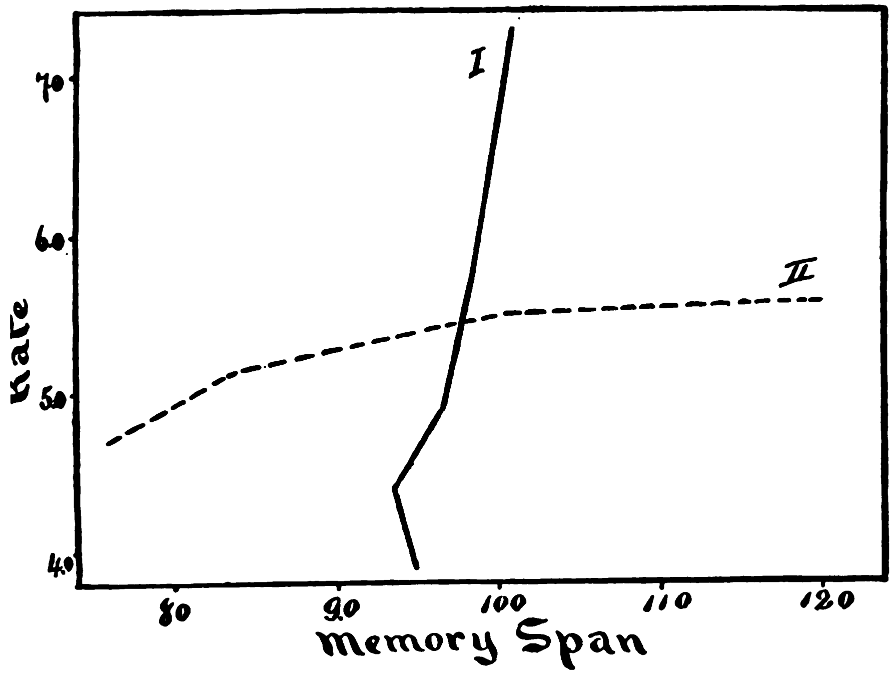
[1] Jacobs, *loc. cit.*

This shows, on the whole, a gradual increase in the rate of reading as the subject moves away from the auditory type and toward the visual. It might be added that slow readers have a lower memory span than rapid. The average total number of words recalled from the auditory, the visual, and the auditory-visual combined test was 16.1 for very slow readers and 20.4 for rapid readers.

Curve 6.



Eye-mindedness is of course quite different from quickness of visual perception. A correlation on the basis of these tests shows absolutely no reciprocal influence.

The errors made in reproducing the lists of words used in this test might furnish material for an interesting chapter in the study of association. Without taking space for details the following deductions might be made: (*a*) Repetitions are frequent—about equally so from the words which are being reproduced and from the list just preceding (in the latter case usually the last word of the group, as that is much more likely to persist). Repetition may be interpreted as a lurking of the memory after-image in the background of consciousness. (*b*) The order is often reversed, as is to be expected from the nature of association. Ebbinghaus and others have shown that the memorizing of a list of words or syllables in a certain order aids in learning it in reversed order: associations are formed backwards. (*c*) In the auditory lists there are many substitutions of a word for two others from which it is compounded phonetically, as 'coal' for 'call' and 'soul,' 'feel' for 'fear' and 'meal.' Variations of this tendency appear in such substitutions as 'deer, foot' for 'door, feet,' and 'race, freight,' for 'rate, phrase.' (*d*) Confusions in sense are less common than those in sound; instances are, 'flour' for 'meal,' 'string' for 'stick.' (*e*) Words are occasionally inserted which have a similarity in meaning to the word which evidently suggests them, being at the same time allied in sound to another of the words used, as 'hay' after 'fork' ('gray' being in the list), 'chain' following 'cord' ('cheer' and 'pain' occurring), 'jest' after 'smile' ('guest' appearing in the same set of words).

3. *Test by detection of differences.*—The second comparison of eye and ear was made by means of the recognition of differences between two variant readings of the same selection. Changes were made by substituting one word for another, usually synonymous; by inserting or omitting a word; and by inverting the order of phrases. Five changes, involving all these classes, were introduced into each of the test passages. The selections used were short—of only 50 words—and of a concrete nature.

In the auditory tests a selection was read aloud to the subject three times in succession (once being found insufficient); then, with no interval between, he listened to the reading of the varied selection once only, being asked to indicate the nature of the differences between this and the first reading. The subject was permitted here to interrupt the reader whenever a change was detected, in order that all the changes might not have to be carried in mind to the end. In the visual series the method was exactly parallel. Instead of hearing the passage read the subject read it himself, silently, three times; then the selection with the necessary changes was given him, and in this he pointed out the variations which he was able to remember. Results are given in percentages in

TABLE VII.

	SUBSTITUTION.	INSERTION.	OMISSION.	INVERSION.	AVERAGE.
Auditory	43.8	96.2	23.6	50.9	53.6
Visual	36.3	67.3	11.3	42.5	39.4
Average	40.1	81.8	17.5	46.7	46.5

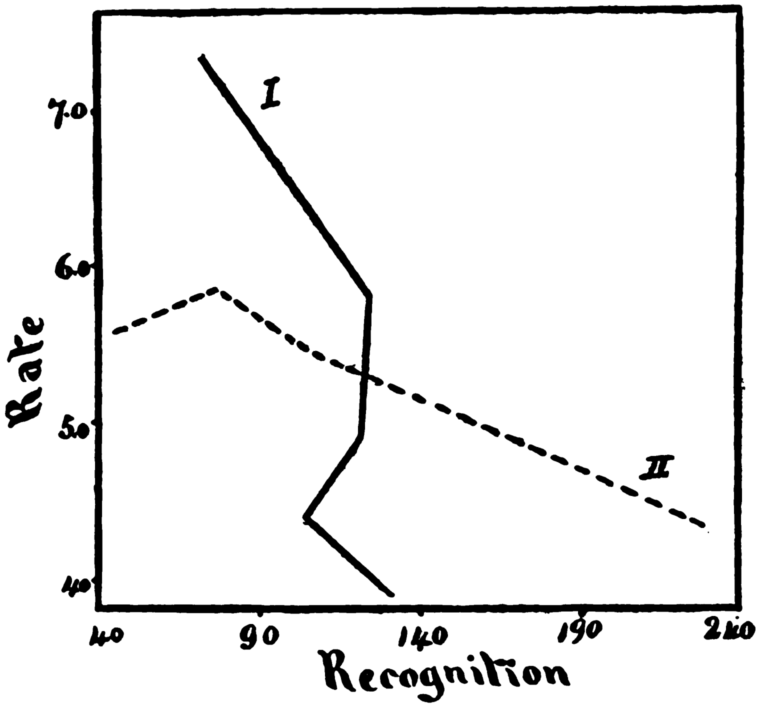
This gives a comparison (*a*) between auditory and visual memory and recognition, (*b*) between the relative recognizability of the different kinds of change made in the passages.

(*a*) Why there should be as great a difference between the auditory and the visual, and with the difference in favor of the auditory, is not at once clear. The passages were read with clear and distinct enunciation, but in the visual tests the subjects were requested to exercise the same degree of care in their silent reading (in each case knowing the purpose of the test). Yet it is doubtful whether they did so; perhaps because in silent reading we are likely to proceed much more rapidly, and, it must be confessed, less carefully. Nevertheless, the result here obtained may be a suggestion that we are not, on the whole, so decidedly eye-minded as has been supposed. This conclusion is supported also by the results of the other comparisons of auditory and visual processes. (Paragraphs 2 and 4.)

(*b*) In the comparison between the kinds of change in the passages it will be noticed that insertions are much more easily recognized than any other change, almost two to one; inversions and substitutions are about equal in the ease with which they are recognized, and omissions are more difficult by far. The reason is not far to seek. If a pen is removed from among half a dozen on our study-table we do not notice the change so readily as when one is supplied. In the latter case the novel object is there to attract our attention; in the former, all we have is a change of relations among the remaining objects, and a new relation is not so easily detected as a new sense perception. Moreover, the subject in these tests was required to tell the exact nature of the change. In the insertions the unfamiliar word was there to be seen and named; in the case of omissions it had to be recalled. Here, as in the other changes, the *fact* of a difference was often recognized and yet the *nature* of it not known. This may be due to memory-images, the sound dimly vibrating, or the vision of a word indistinctly hovering, in the margin of consciousness. Changes not present were sometimes introduced and somewhat more frequently in the auditory than in the visual tests.

The position, in the selection, of the different changes would have an influence. Those occurring near the beginning or the end would be more likely to be remembered, as 'primacy' and 'recency' are recognized conditions of suggestibility.[1] The different sorts of change were therefore distributed somewhat evenly throughout the selections. Vividness might also be a factor in the suggestiveness of certain changes; one substitution,

Curve 1



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[1] See Miss Calkins on *Association*, a Monograph Supplement to PSYCH. REV., Feb., 1896.

for example, might be more impressive than another. Visual and auditory series, however, were made in every point as nearly comparable as possible.

The comparison of reading rate with the predominance of eye-mindedness gives a result quite different from that obtained by the memory-span test. Here the most rapid readers are those who incline most strongly toward the auditory type, as the table and the curve (7) show.

TABLE VIII.

RECOGNITION OF CHANGE ON THE BASIS OF READING RATE (I.).

Class . .	XX	X	M	A	AA
Rate . . .	3.9	4.4	4.9	5.8	7.3
Change .	130.38	105.01	122.12	122.17	71.66

READING RATE BY RECOGNITION OF CHANGE (II.).

Change .	42.4	77.7	103.37	148.13	227.39
Rate . . .	5.56	5.89	5.47	5.05	4.36

The lines incline more or less towards the N.W.–S.E. diagonal of the rectangle. Eye-mindedness, as determined by this test, is therefore detrimental to rapidity of reading. The interpretation of this is doubtless very simple: rapid reading is not favorable to the detection of slight differences in form or meaning. The rapid reader, acquiring the habit of looking only for essentials and disregarding unimportant details, detects minor variations less readily in written than in spoken sentences. But though rapidity of reading is not conducive to the recognition of differences through the eye alone, yet the sum of the changes recognized by eye and ear is 40% greater for very rapid readers than for the slow.

4. *Method of Comparison by Simultaneous Reading and Hearing.*—Eye and ear were compared lastly by the number of thoughts reproducible from the material acquired through these sensory avenues. Two selections, each of about 150 words, were read simultaneously, the one silently by the subject, the other aloud by the experimenter.

As before, the material used was of a concrete nature and easily picturable, as anything abstract would have been entirely too difficult for a test under such conditions. The subject read his extract at about the same rate as the other selection was read to him, the ending of the two readings as well as the beginning being thus simultaneous. The subject then repeated as many of the thoughts as he was able to recall of each selection, and, so far as possible, in the same words. A complete record of this was taken in shorthand by the experimenter. The advantage of this method over that of having the subject write the reproduced thoughts will be readily seen: a much shorter interval elapsed between the reading and the completion of the reproduction, a truer comparison being thus given between auditory and visual powers. Even by this method, in the short time required to reproduce one selection (auditory), much of the other (visual) would be forgotten. As a corrective of this a second test was made, with another pair of selections, and the order of reproduction reversed; that is, the visual passage was first recalled and afterwards the auditory. Auditory and visual tendencies were compared by the number of 'thoughts' reproduced from the original selections, these thoughts being divided into 'important' and 'minor.' A record was also kept of the number of 'extraneous thoughts' introduced. Not only was the correctness of reproduction considered but also the quality, in general as well as with regard to expression and to logical content. This is important in the calculation of the value of the reproduced material, as it might very well happen that one person would recall more thoughts than another whose reproduction on the whole, was much more creditable. In estimating quality, five grades—very good, good, fair, poor, and very poor —corresponding to those under visual perception, were taken into account.

This method of experiment would, of course, be in no sense a test of the relative powers of eye and ear if the subject intentionally gave more attention to one process than to the other. He was asked, however, to remember as much as possible of *each* selection, and the results show that in no case was the request disregarded. This test was an exceedingly difficult one, inasmuch as voluntary attention to two entirely distinct series of impressions, each involving for its interpretation very complex processes of thought, is quite impossible. The 'wave' of consciousness has but one crest. The 'doubling' of the mind is nearest approached in its "simultaneous application to two easy and heterogeneous operations; two operations of the same sort—two multiplications, two recitations, or reciting one poem and writing another—render the process more uncertain and difficult."[1] The processes in the present test were neither easy nor heterogeneous, involving, as they did, not only the understanding, but the memory also, of the passages read and heard. The *sensory* processes, merely as such, could very easily go on simultaneously, one being auditory and the other visual. In the attempt, however, to focus attention upon two unrelated series of *ideas*, what actually took place, as testified by many of the victims of this experiment, was a flitting of attention from one to the other. The gaps thus resulting in the continuity of each series were repaired all too frequently by the imagination. Many spoke of a 'blacksmith' with 'curly locks' hanging down to his shoulders, the original selection being a description of a locksmith's shop with numerous locks hanging about the walls. The same idea was further followed out by such phrases as 'brawny arms,' 'honest sweat,' 'children passing,' all evidently called up from Longfellow's poem by unconscious association.

5. *Results of comparison by simultaneous processes.*—As in the other tests of eye and ear the auditory process has an advantage, though in this case it is very slight, the average reproduction of the material *heard* being 18.6 % of the total number of thoughts in the selection, while the percentage reproduced from the material *read* was 17.1. On the other hand, the quality of reproduction was slightly (4.3 %) higher by the visual method. The number of thoughts introduced which were not in the original selection was practically the same for both processes—in the auditory 4.2 % of the total number of thoughts reproducible from the selection; in the visual, 4.6. This tends to show that sound- and sight-images, in marginal consciousness, are equally liable to misinterpretation.

The correlation between rate of reading and visual-auditory ratio gives a result similar to that obtained from the memory-span experiment, namely, that eye-mindedness accompanies rapid reading. The curves (8) and the averages from which they are drawn are as given:

TABLE IX.

EYE-MINDEDNESS BY READING RATE (I.).

Class	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Eye-mindedness	89.08	106.13	84.8	97.77	123.2

READING RATE BY EYE-MINDEDNESS (II.).

Eye-mindedness	57.0	78.7	96.8	114.2	154.1
Rate	5.25	4.99	4.82	5.61	5.95

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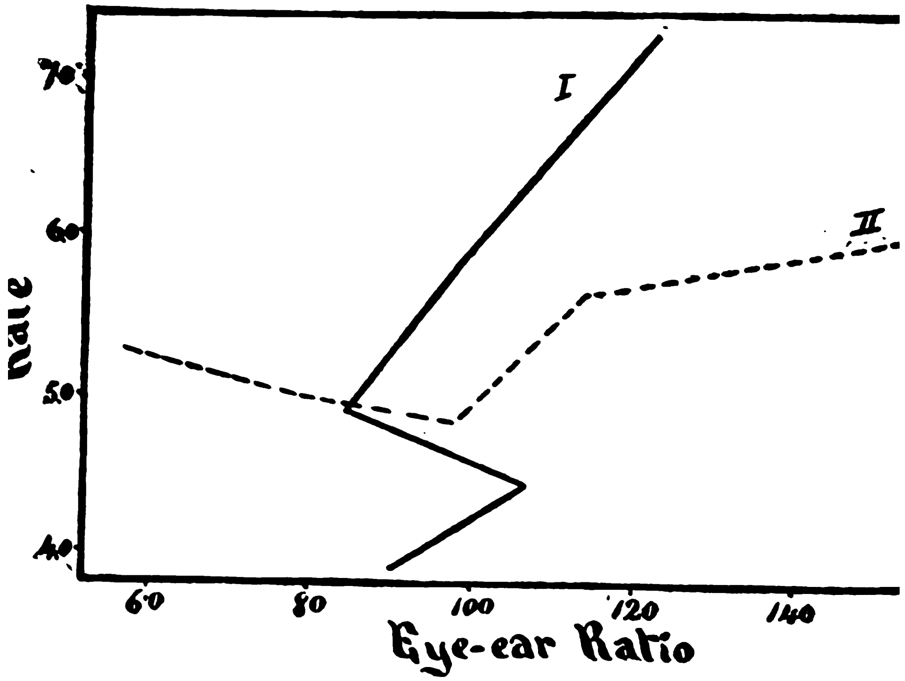
[1] Paulhan, *Rev. Scientifique*, Vol. XXXIX., p. 684. Quoted by James.

The irregularity of these lines is due to the combination of two causes: the small number of records included (50), and the very difficult and complex nature of the experiment. The more complex the process the greater becomes the probability of accidental variations, and the larger, therefore, the number of records which must be included if the influence of chance is to be excluded.

Curve 8



The degree in which the rapid readers excel the slow in eye-mindedness can perhaps best be understood by a comparison of the extreme classes. The 'very slow' readers (3.9 words per second) reproduce 89.1% as much of the visual selection as of the auditory, while the 'very rapid' readers (7.3 words per second) are able to recall 123.2 of visual for every 100 of auditory; that is, the ratio of reading rates between slowest and fastest readers is 3.9 to 7.3 (1 : 1.87) while the ratio of the visual tendency as compared with the auditory is 89.1 to 123.2

(1:1.38). On the principle of correlations this result shows eye-mindedness to be a rather strong factor in the determination of reading rates.

It might be supposed that greater rapidity was gained at the sacrifice of exactness or of intelligence. This supposition is negatived by an examination of the amount and quantity of the material reproduced. A comparison between the ten most rapid readers and the ten slowest shows that the rapid readers remember more of the original thoughts, and that the character of their reproduction is much higher, both generally and with reference to expression and to logical content. In the auditory tests the ratio of slow to rapid readers is 14.8% to 20.7%, in the *number* of thoughts. In *quality* the percentages are 47.8 for slow readers, 60.3 for fast. The same comparison in the visual tests results as follows: percentage of thoughts reproduced by slow readers, 14.9; by rapid, 24.4. Quality: slow, 48%; rapid, 73.3. The difference in favor of the 'rapids' is consequently much greater than in the auditory tests, indicating again that rapid readers are, as a rule, of the visual type.

Combining both quality and amount of reproduction and both auditory and visual tests, thus making the comparison general, and continuing it throughout the different classes, we get the following numbers and curve (9) which show an increasing advantage to the rapid reader.

TABLE X.

RATE DETERMINING TOTAL REPRODUCTION (I.).

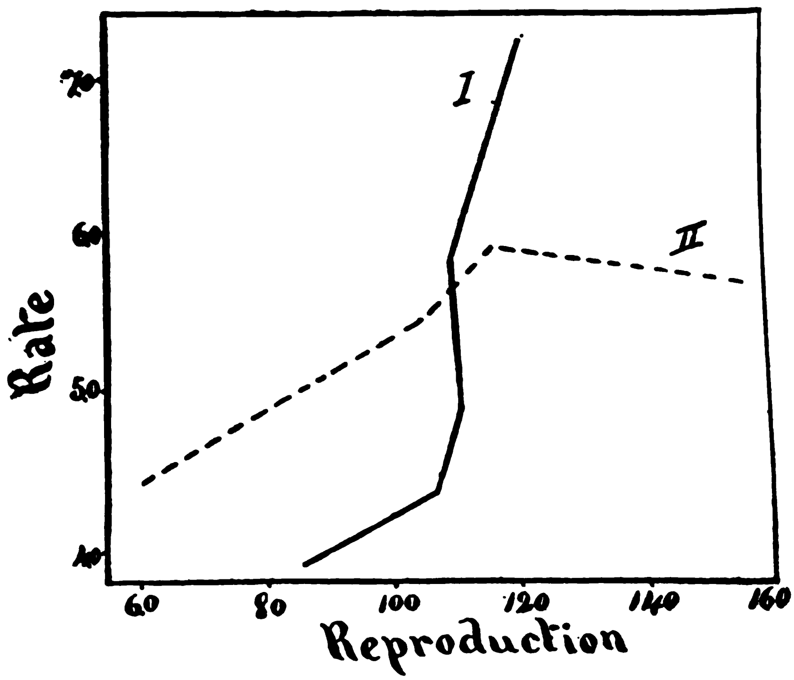
Class	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Reproduction	85.3	107.0	110.5	109.0	119.9

REPRODUCTION DETERMINING RATE (II.).

Reproduction	60.0	89.3	104.3	115.6	158.2
Rate	4.42	5.15	5.45	5.91	5.64

In this test where eye and ear were used simultaneously, there is the possibility of rapid readers having a greater relative advantage than in recollection under ordinary circumstances. The effort of the subject in trying to grasp the meaning of both passages at the same time resulted, as already stated, in a rapid alternation of attention between the two. The rapid reader would, therefore, have an advantage in being able more easily than the slow reader to catch up his own reading after his attention had been following the passage read to him. Nevertheless, both the preceding auditory-visual tests show the same result, and to a striking extent. In the memory-span test the rapid reader was superior to the slow by 26.7% ; in the test by recognition of variations in the different readings the percentage was 43.7 ; in the present test, of simultaneous processes, 40.5, (119.9 is 40.5% in advance of 85.3). These give an average of 37% by which the rapid reader is superior to the slow in quality of work. Among the causes of this result are probably differences in general intelligence.

Curve 9



The superiority of the rapid reader is also shown by the fact that his memory of the substance of his reading is more exact than that of the slow reader. He introduces only two-thirds as many thoughts not found in the original selections.

As measured by col. grades, not much relat

6. *Relation of intellectual ability to reading rate.*—Whether the supposition just made, that the degree of intelligence exercises some influence on rapidity of reading, is true to any considerable extent, is difficult to verify, since all tests of mental capacity are inadequate and unsatisfactory. Sensitiveness and sensibility have been suggested as tests. But these are only receptive powers and inferior to expressive. The dog, whose sense of smell is so much finer than our own, or the cat, whose acuteness of hearing surpasses ours, or the eagle, whose vision is many times keener, are not therefore our mental superiors even in these respects. Early youth is more sensitive than manhood, but does not possess greater mental capability, nor should we say that one's powers of mind had been greatly increased by an extended series of experiments which heightens sensibility in any of the senses. There is an intellectual as well as a sensational use of the senses.

External tests are even less satisfactory. The weight of the brain, or its volume, is a measure of capacity only when we add the limiting phrase, 'other things being equal.' But the latest researches in cerebral anatomy and physiology are far from enabling us to calculate the variety and importance of these other conditions.

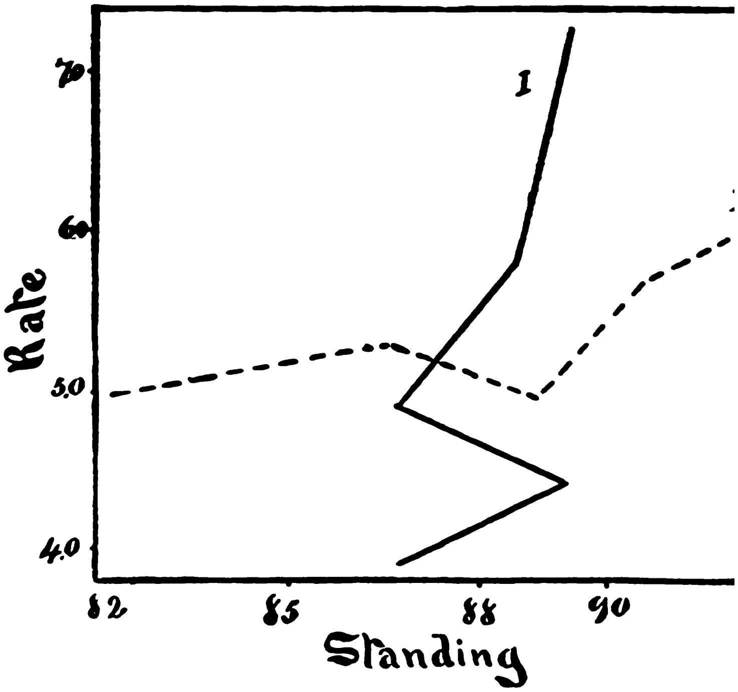
Any adequate estimate of the individual mind must doubtless include feeling and will, along with intellect, yet scientifically accurate tests of these are not easy to suggest.

Class standings are doubtless not a true test of even intellectual strength; geniuses have not been distinguished for brilliant work at school. College records, however, are a means by which we can judge scholarly ability, and the class standings of the students who were subjects of these experiments are here compared with their reading rates. The result (Curve 10) does not show a close relationship.

7. *Major and Minor Thoughts.*—In the thoughts recalled in the 'simultaneous test' the auditory ratio of minor to major was nearly double the visual—24.8% and 12.9% respectfully. This does not necessarily mean that under ordinary conditions there is less distinction between the important and the unimportant when one hears a passage read than when one reads it himself. It does mean, in all probability, that such is the case under the conditions of this experiment; namely, when the attention is divided. It is true that the

reading aloud to the subject was intended to be rather unexpr
as anything else would have defeated the main purpose of th
It may be, therefore, that when one listens to expressive readin
gives the whole attention to it, the foregoing discrimination in f
the visual process will disappear, and the important thoughts
ceive, in the selection heard, as great a relative attention as in t
sage read, to the comparative disregard of the less important.
cedently one would be inclined to pronounce in favor of the au
for the printed words are no more impressive, as mere symbols
conveying a weighty thought than when only a trivial one; b
tones of the human voice, on the contrary, are much more i
sive, being capable of great modulation to express shades of m
and emotional qualities.

Curve 10.



§ IV. The Motor Type and Lip-Movement.

1. *Method.*—Along with the eye and ear types goes an
less known but not less interesting, and, from certain poi
view, not less important—the motor type. Persons of thi
think in terms of the muscle sense—their images are
graphic.' In silent reading they are, as a rule, lip-mov

t least imagine themselves speaking the words. It is especially in relation to lip-movement and its implications that this roup is here studied.

The experimental method of determining motor readers was s follows: Three passages of equal length and difficulty were ead silently by the subject, all at the maximum rapidity at which he could read intelligently. The first was read under normal conditions; that is, with no external restrictions. During the reading of the second the subject was required to hold the tip of his tongue underneath his upper lip, thus interfering greatly with the free movement of his lips, if he were a lip-mover, and, consequently, interfering with his attention, and thereby decreasing his reading rate. If he were of the motor type, yet not a lip-mover, this position of the tongue would still interfere with his imagining the position of the vocal organs, just as it is difficult, when lying down, to imagine one's self running, because the position hinders tendencies to movement which accompany the idea of running. By way of comparison with this second test a third selection was read under conditions which introduced an equal interference with attention but not with lip-movement—the subject kept up an approximately uniform pull on a dynamometer, varying in amount with different individuals, according to their strength.

Other methods of determining lip-movement were employed: observation by the experimenter (unsuspected by the experimentee), as well as the latter's own judgment, expressed after he had taken a few days to study introspectively his own reading habits. (See appendix, 'Personal Sheet.')

2. *Results.*—The results obtained by these methods agree fairly well. That they do not agree perfectly arises from the facts (1) that one's judgment of his own methods of reading, unless he be a somewhat skilled self-observer, may be at fault, and (2) that the observation by the experimenter was confined o too few tests of the reader's habits to be conclusive. In the ollowing table are given in percentage the subject's own estimate f his lip-movement in general, and also the observation of it by he experimenter in particular kinds of reading. The column esignated 'careful reading' is the case in which the subject

read very attentively with a view to detecting variations in the two readings of the same selection (the second comparison between eye and ear, above). 'Normal reading' means simply ordinary silent reading under normal conditions. 'Rapid' refers to the maximum rate of intelligent reading.

TABLE XI.—LIP-MOVEMENT.

	OBSERVATION BY EXPERIMENTER.				SUBJECT'S OWN ESTIMATE.
	Careful Reading.	Normal Reading.	Rapid Reading.	Average.	
Decided Movement .	25.5	0	5.7	10.4	2.1
Medium Movement .	35.3	28.6	24.5	29.5	35.4
No Movement . . .	39.2	71.4	69.8	60.1	62.5
Total	100	100	100	100	100

The average as observed thus corresponds pretty closely to the subject's own estimates, the differences here, as elsewhere, indicating that the subject is inclined to be somewhat generous to himself.

3. *Nature of lip-movement.*—To gain an understanding of the real nature and significance of lip-movement in silent reading we must know something of its origin. To answer the theoretical question of how it arises may give us a deeper insight into the nature of it, and thus aid us in answering the practical question of how to deal with it. The argument can be most definitely focused by marshalling the facts about a conclusion stated somewhat as follows: Lip-movement in silent reading is not an acquired habit, but a reflex action, the physiological tendency to which is inherited. It is not 'second nature' but essentially first nature; not something to be *unlearned* but to be *outgrown*. It is a specific manifestation of the general psycho-physical law of 'dynamogenesis' by which every mental state tends to express itself in muscular movement.

In support of this the following considerations are adduced:

(*a*) The child in learning to read does not learn to move his lips. The lip-movement is most decided at the very beginning, and grows less so, as he becomes conscious of it and controls it voluntarily. In learning to write, also, the child moves his tongue and the muscles of his face; sometimes even his feet or his whole body. But he afterwards finds that this expenditure of ,energy is unnecessary. It is brought under control when the writing itself becomes partially reflex, leaving the higher conscious processes more free to attend to the inhibition of these lower useless ones. The same act is then performed more exactly, more quickly, and with less exhaustion. Similarly, lip-movement is an unnecessary expense of energy — not only useless but detrimental.

(*b*) In the answers to the 'personal sheet' many persons say that though not habitual lip-movers they do move their lips when giving very close attention, or when reading matter which is very difficult, absorbingly interesting, or highly emotional. This means simply that we regularly inhibit these vocal reflexes, but that when our whole attention is given to the thought under consideration the watchfulness over these motor tendencies is relaxed, and they find expression. We tend to 'think aloud' when pre-occupied — not only lip-movement but actual speech is unintentional. It is true also that these impulses to vocal movement are stronger, and hence more likely to find an outlet, when the corresponding mental processes are more vivid. This follows from the general principle, experimentally demonstrated by Féré,[1] that "the energy of a movement is proportional to the intensity of the mental representation of that movement."

(*c*) Some students state that lip-movement aids concentration and comprehension. This is negatived by the comparison of lip-movers' work with that of non-lip-movers (Curve 13, below). Instead of lips being moved as an aid to close attention, the causal relation is in the opposite direction: the close attention is the cause, or rather the condition, of the lip-movement (as in *b*, above). Careful introspection will verify this; many of us who are decidedly not 'motaires,' will often 'catch' ourselves moving our lips and tongue while we are struggling with a paragraph from Kant or Lotze. That is, the beginning of the movement is not conscious and deliberate. It is therefore an aid to concentration only negatively—by the expression of itself in a natural way it leaves the whole attention free to be focused upon the immediate content of consciousness. It ought to be added that lip-movement is doubtless an indirect aid to comprehension and retention from the mere fact that it lengthens the *time* of reading. (Curve 11.)

---

[1] *Sensation et Mouvement.*

(*d*) This interpretation is supported by the results of observation, given in Table XI. In careful reading when the fullest attention is given to the thought a higher percentage of decided lip-movement is shown than in ordinary or in rapid reading, and those who show no movement are a very much lower percentage. At first view we should be inclined to say that in rapid reading also the inhibitory process would be completely relaxed and the motor tendencies would express themselves strongly. But this is shown only slightly as compared with the 'normal' column. The reason is doubtless that in reading at a maximum rate we employ the visual process almost exclusively, and we cannot easily perform even the beginnings of articulation so rapidly.

(*e*) The statement that lip-movement is 'natural,' and reading without lip-movement an acquired habit, is entirely in accord with mental laws. The development of mind is not only in handing over processes, once conscious, to the control of the reflex mechanism—as in walking or in playing a familiar tune on the piano—thus leaving consciousness free for the acquisition of higher powers and the performance of tasks more difficult; but the interchange is also in the opposite direction—originally reflex processes are frequently brought under the control of the higher consciousness, and inhibited if they are considered useless or detrimental.

(*f*) The mental growth of the child is in general parallel with the growth in culture from primitive to highly civilized races. The close connection between thought and action is best seen in children and in uncivilized peoples who have not learned to repress the motor accompaniments of thought and emotion. As we rise in the scale of intellectuality people become less demonstrative, until we reach the highly educated and intellectual, who are proverbially 'cold.'

(*g*) A supplementary argument might be drawn from the 'law of reversion,' by which the latest acquired is the earliest to be lost, in diseases of memory or in senile dementia. Innate physiological tendencies, therefore, would be the last to go. Observation shows the tendency to lip-movement to remain when the power to use language intelligently has been lost.

This position is not invalidated by the fact—if it be a fact—that "every child, if removed early from its parents, can learn to master any language whatever, instead of its mother tongue, and just as perfectly as that."[1] The inherited physiological tendency may be toward the use of speech without being toward any one language in particular until the child begins to use language.

The pedagogical value of this interpretation of lip-movement is evident. On our understanding of the child's mind depends our intelligent direction of it. Knowing that certain practices, instead of being mischievous habits, acquired through carelessness and deserving of censure, are but the natural expression of inevitable physiological tendencies, we shall be able to deal with them more sympathetically. It may be desirable that the child or the adult should escape from the retarding influence of lip-movement in reading; knowing the nature of the imperfection will enable him more wisely to direct his efforts in outgrowing it.

4. *Correlation of Lip-movement with Reading Rate.*—The relation between rate and lip-movement is seen in the following table and correlation. (Curve 11.)

MOTOR TENDENCY DETERMINED BY READING RATE (I.).

Classes	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Motor	8.4	6.6	5.3	4.7	4.5

RATE BY MOTOR TENDENCY (II.).

Motor	3.0	4.9	6.7	9.0	11.7
Rate	5.6	5.5	5.4	4.6	4.3

This shows that the motor tendency in any degree has an influence detrimental to rapidity of reading, and the stronger the tendency the greater is the hindrance; the numbers representing the lip-movement regularly decrease as reading rate increases, and *vice versa*. The curves, so far from having any tendency to approach the (N.E. – S.W.) diagonal of the rectangular plot, toward which they would tend if lip-movement were a furthering influence in speed of reading, cross this diagonal almost at right angles, thus demonstrating clearly the fact that the reciprocal influence is adverse.

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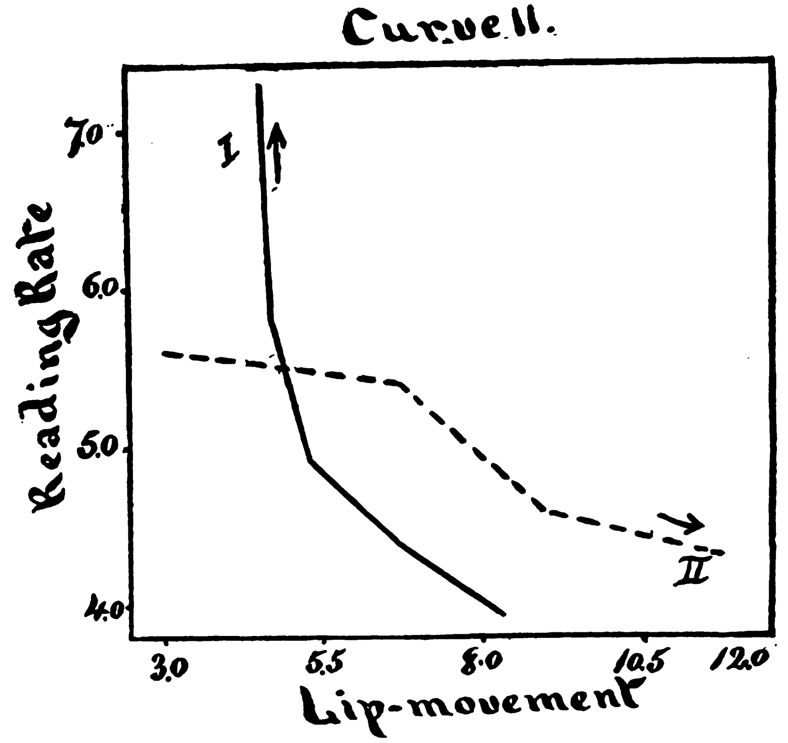
[1] Preyer, *Infant Mind*, p. 121.



To emphasize this relation a comparison of extremes might be shown as follows: 'The ten slowest readers show almost double the amount of lip-movement that the ten most rapid do. Or again, determining rate by means of lip-movement, we have: the ten most decided lip-movers read 4.1 words per second; that is, they are between the classes 'slow' and 'very slow,' and nearer to the latter; while the ten who show least movement of lips read 5.6 words per second,—very close to an average 'rapid.'

When the visual tendency is combined with the motor the lip-movement is not so decided as in the auditory-motor combination.

5. *Lip-movement and Extent of Reading.*—Bringing together this result of the relation between lip-movement and reading rate, and that reached later (Curve 16) between amount of practice and rate of reading, we should expect that lip-movement and extent of reading from childhood would be in inverse ratio. That is to say, the ratio between rate and lip-movement being inverse, and that between rate and extent being direct, the ratio of lip-movement to total amount of reading ought to be inverse. A direct comparison shows this to be true. (Curve 12.)

TABLE XII.

LIP-MOVEMENT AND EXTENT OF READING (I.).

Class	XX	X	M	A	AA
Reading		25	30	35	40
Motor		7.7	6.2	5.7	3.0

READING BY MOTOR-TENDENCY (II.).

Motor	3.0	4.9	6.7	9.0	11.7
Reading	33	29.4	28.3	31.5	26.7

The lowest parts of these curves run almost exactly in opposite directions, as do also the highest. The interpretation of this is, that extent of reading works directly against movement of lips, and is practically the only thing which does so, except among the medium lip-movers, where other factors seem to enter. The general result here obtained is strengthened by the observation that all *AA*'s in amount of reading are *XX*'s in motor tendency; that is, not one of those whose reading is widest is a lip-mover to any extent which can be observed.

6. *Relation between motor-mindedness and quality of intellectual work.*—In the test between eye and ear by means of simultaneous processes (§3, Par. 4.), the more distinctly a subject belonged to the motor class the smaller the amount of material was he able to reproduce from the selections. All the thoughts recalled from both the auditory and the visual selection, together with their relative value (estimated by a necessarily conventional standard) were added for each subject

separately, and this sum was compared with the strength of his motor-mindedness. The former increased regularly as the latter decreased, as shown in Table XIII and Curve 13.

Curve 12.

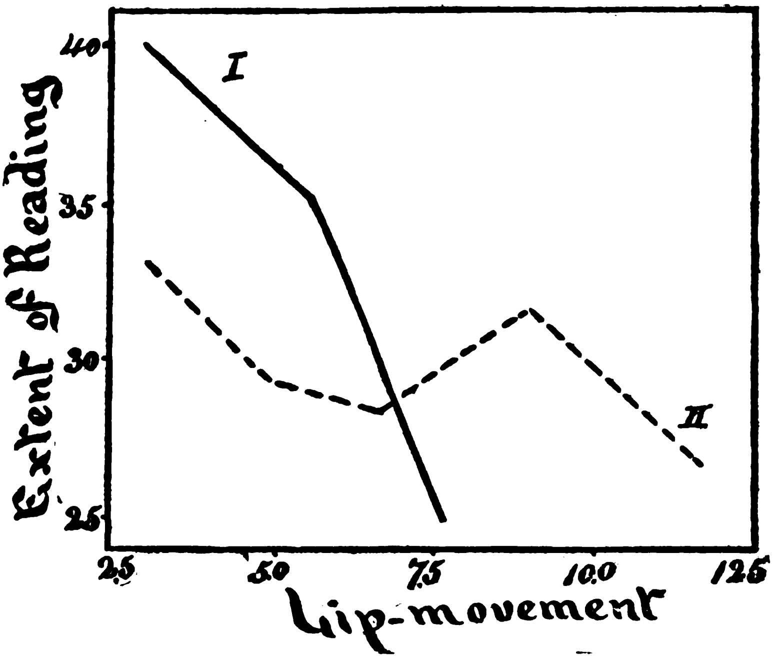


TABLE XIII.

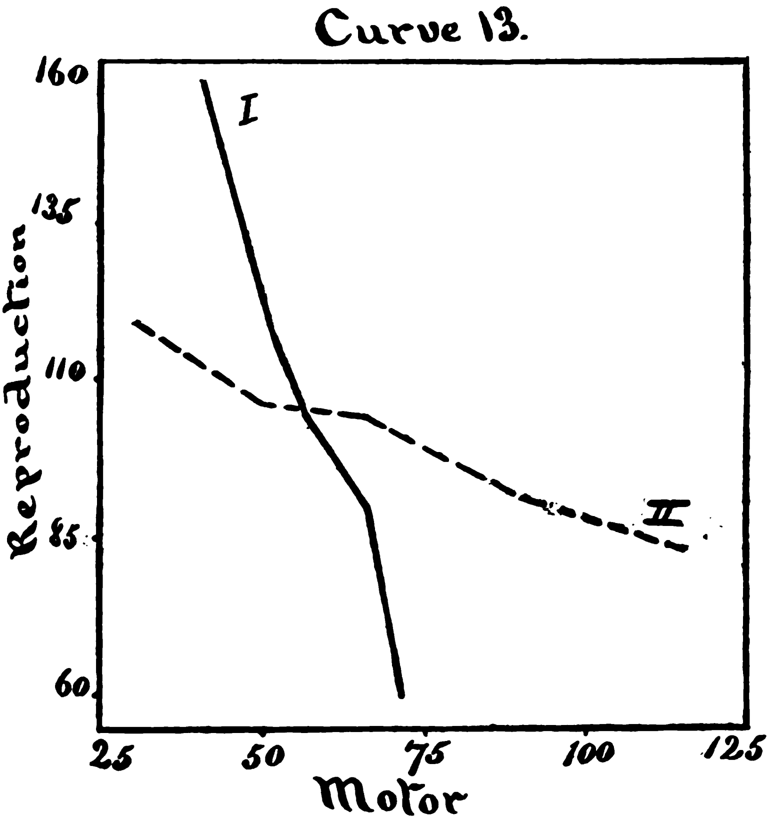
MOTOR-MINDEDNESS AS DETERMINED BY REPRODUCTION. (I.).

Class	XX	X	M	A	AA
Reproduction	60	89.3	104.3	115.6	158.2
Motor	7.2	6.7	5.7	5.2	4.2

REPRODUCTION BY MOTOR-MINDEDNESS (II.).

Motor	3.0	4.9	6.7	9.0	11.7
Reproduction	118.0	106.7	104.8	91.8	83.8

This, in itself, does not prove that motor-minded persons are less intelligent readers than auditory- or visual-minded. As has been shown, those of the motor type are slow readers, and are probably, therefore, at a greater relative disadvantage in a test of this kind than in any test under ordinary conditions, where only one passage is read at a time and afterwards recalled. Still, it is evident that motor-mindedness is far from being an advantage to a reader, either as regards the time required for reading or the amount obtained from it.



§V. Other Determining Factors of Rapidity of Reading.

1. *Agreement of Personal Judgment with Experiment.*—From the answers to questions of the 'personal sheet' other factors influencing rate of reading were determined. These answers are probably less exact than scientific experiment would furnish; yet a comparison between the two sources of information shows a closer relation than might be expected. The tests of reading rates were compared with the students' own judgment of the same. In a few cases the personal equation is

quite strongly marked. One man calls himself a 'very slow' reader whose rate in reality is higher than that of another who classes himself as 'medium;' another estimates his speed as 'rapid' whose rate is below that of one who calls himself 'slow.' But these are the rare exceptions; as a rule there is great uniformity between the personal judgments and the literary tests; for example, all those who class themselves as 'slow' readers, with a single exception, are found by test to be within the limits of four to four and a half words per second. The comparison throughout stands thus:

TABLE XIV.

Class	XX	X	M	A	AA
Rate according to experiment	3.9	4.4	4.9	5.8	7.3
Rate according to self-judgment	4.2	4.3	4.9	5.9	7.8

These figures show the average rate for each class; that is, 3.9 words per second is the average rate of those who are classed according to the experiments as 'very slow,' and 4.2 is the average rate, by experiment, of those who class themselves by their own judgment as 'very slow;' and similarly for the other grades. The correspondence between the two methods of classification is thus seen to be very close, except in the extreme classes. The reason for the divergence here is (1) that in the lowest class the number of individuals who are willing to call themselves 'very slow' is so small that the figures are not representative, the variation of 'judgment' from 'experiment' being merely accidental, (2) the most rapid readers show a characteristic modesty in hesitating to place themselves in this highest class. This raises their average above that of the classification by experiment.

2. *Mental Alertness and Reading Rate.*—Taking alertness of mind to be correctly estimated by one's own judgment of his degree of rapidity in composition, the curve connecting this with reading rate is given below. (Curve 14.)

Rapidity of composition is probably a more adequate test of the quickness of mental process than most laboratory tests would be. Multiplication and similar mathematical tests depend too much on practice; 'finding time' involves too large a physiological element, and complex reaction-times are necessarily too specific to furnish a true test of general quickness of mind.

3. *Mental concentration and reading rate.*—Obviously one can perform mental operations more rapidly when the mind is strongly concentrated on the work than when the attention is divided. Power of concentration was therefore compared with speed of reading, with the following result, giving Curve 15.

Curve 14.

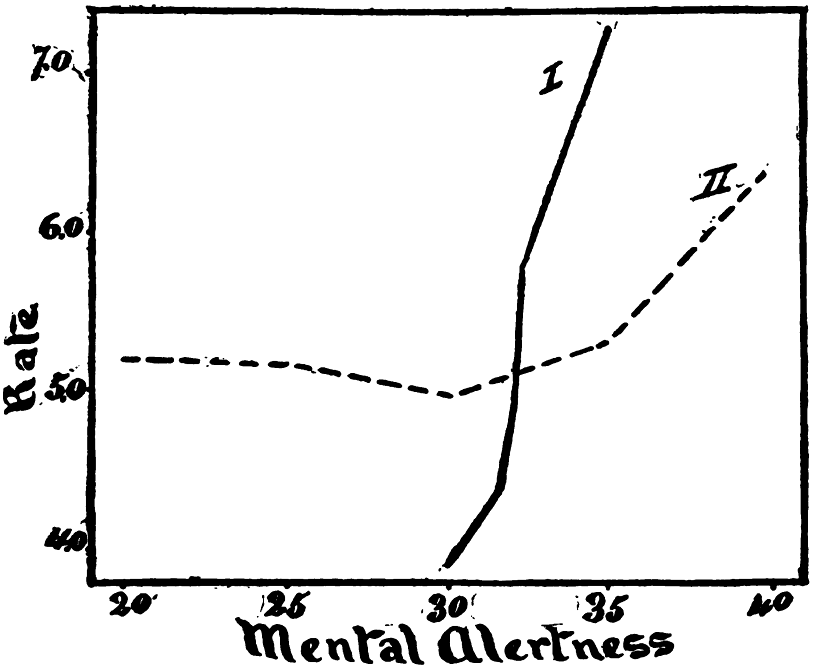


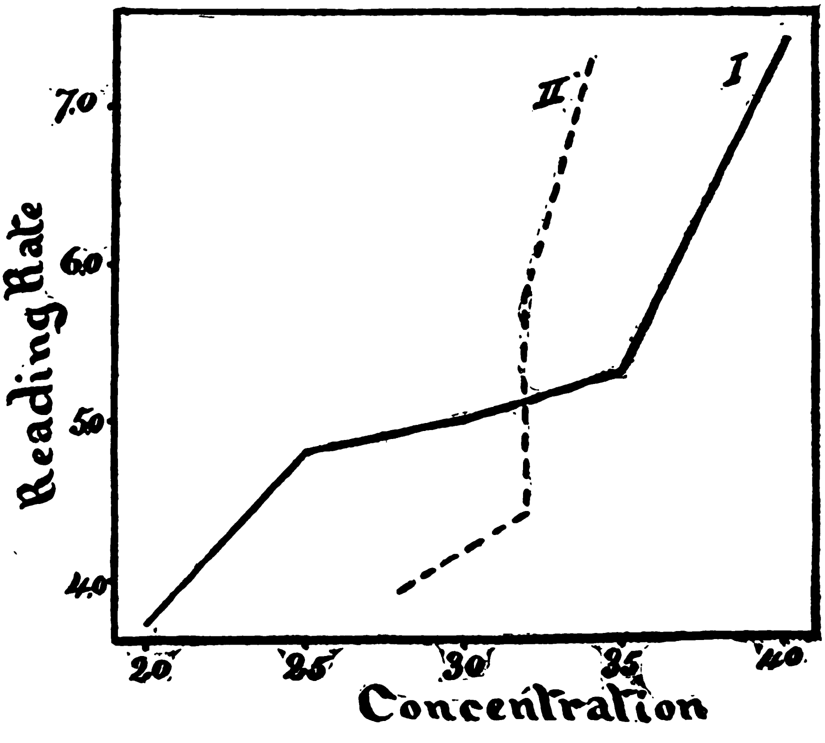
TABLE XV.

READING RATE DETERMINED BY POWER OF CONCENTRATION (I.).

Classes	XX	X	M	A	AA
Concentration	20	25	30	35	40
Rate	3.7	4.8	50.0	5.3	7.4
CONCENTRATION BY RATE (II.).					
Rate	3.9	4.4	4.9	5.8	7.3
Concentration	2.8	3.2	3.2	3.2	3.4

The lower ends of the curves have the same general direction. This evidently means that poor concentration tends to hinder rapidity of reading. The curves, moreover, are nearly parallel at the upper ends; that is to say, high reading rate accompanies unusually good powers of concentration. On the other hand, the curves cross almost at right angles; consequently there is little relation between rate and concentration in the three middle classes. In a word, the greatest and least degrees of concentration affect reading rates favorably and adversely, but greater or less degrees do not.[1]

Curve 15.



4. *Practice and reading rate.*—Practice was also tested as a possible factor in rapidity. Here again the subject's own judgment was necessarily taken. Information as to the extent of his reading from childhood up could be obtained in no other way. The result (Curve 16) shows a rather close correlation.

---

[1] This conclusion is merely tentative. In a thorough study of the influence of concentration it would be necessary to consider the relation between the sensory type of the subject and the nature of the distraction, *e. g.*, an 'auditaire' would be easily disturbed by noises, a 'visionaire' by sights.

TABLE XVI.

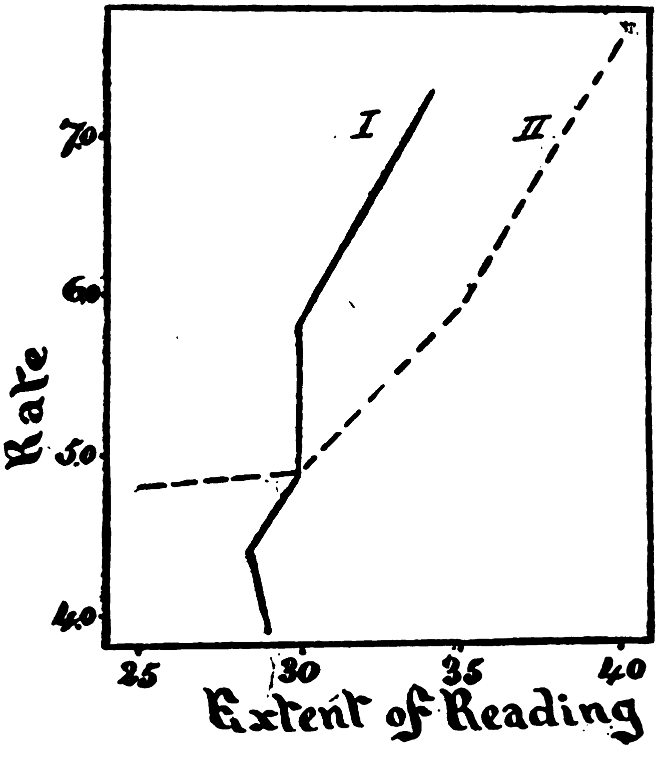
EXTENT OF READING DETERMINED BY READING RATE (I.).

Class	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Extent	29.0	28.5	30.0	30.0	34.1

RATE BY EXTENT (II.).

Extent	25	30	35	40
Rate	4.8	4.9	5.9	7.6

Curve 16.



Practice always plays a large part in the earlier stages of perations which involve a combination of muscular and mental ctivity; copying, for example, or shorthand writing. But one an reach a point beyond which the effect of practice is indefiitely small. This, however, is usually because the *physiologiul* limit of quickness has been reached; and if reading were to ny great extent a physiological process we might well suppose all college students to have passed the point at which practice ceases to have an appreciable effect. But these considerations apply only to operations in which we have made conscious and continued efforts to reach the highest skill. Reading, however, is only slightly physiological, except, of course, in so far as all mental operations are conditioned on the activity of brain cells. So it comes about that practice continues to show a marked effect even in those subjects whose extent of reading is very large. Word-recognition is more and more fully handed over to the centres governing reflex action, and thus the time required for the process of recognition is decreased. The reflex arc is shorter than the higher conscious circuit, and, moreover, nervous impulses travel faster through fibres than through cells.

§ VI. THE RELATION OF EYE TO VOICE IN READING ALOUD.

A question of some importance remains: When one reads aloud how far does the eye ordinarily travel in advance of the voice? This is a practical query with regard both to *intelligent* and *intelligible* reading. If the eye is only two words ahead of the voice, in reading new matter, the reader is not likely either to understand the thought—without returning upon it—or to make a listener understand it. Moreover, with the large number of words in English which are identical in spelling but different in pronunciation and meaning, unless the sight and the understanding are somewhat in advance of the voice, mispronunciations will be frequent.

An attempt was made to ascertain the factors contributing to this difference of position between eye and voice. The number of words perceived but not yet spoken at any given point in the reading was determined by a very simple method. At definite points in the reading, previously decided upon but unknown to the reader, a card was quickly slipped over the page, and the number of words spoken after the view was thus cut off was recorded. Of *objective* influences might be mentioned the legibility of the words (depending upon spacing, 'leading,' size of type, color of paper, etc.), the length of the lines, the position in the line at which the interruption took place, and the effect of punctuation. These factors were little studied, as our nterest was mainly in the mental and not the external conditions.[1] An octavo volume was used (length of line 3⅝ inches), the matter being of ordinary difficulty and containing no technical words. In the first trial the subject did not know the purpose of the experiment, and it was quite unexpected by him when the page was cut off from his view. Of course he could be surprised in this way but once. Succeeding trials, however, under the same objective conditions and with only the subjective difference that he knew how he was to be treated, gave results not appreciably different. Much depends upon the position in the line at which the view is intercepted. When the reader is pronouncing a word at the beginning of a line, the eye is on an average 7.4 words in advance of the voice; in the middle, 5.1 words; and at the end, 3.8, giving an average of 5.4 words. Thus the space between eye and voice is very elastic, expanding and contracting with each line, but with a uniform regularity,—except indeed where special conditions are introduced; an unfamiliar word, for instance, would decrease the distance to zero, or a familiar phrase might increase it to a dozen words. The fact that a new or strange word calls a halt to the eye's advance illustrates the well-known principle that we cognize familiar words as a whole, and do not 'see' each letter separately; misprints usually pass unnoticed except by the proof-reader.

The distance between the words which the voice is uttering and the point of regard being so variable for different parts of the line, the number of words by which the eye precedes when we commence the utterance of a sentence is not comparable with an average of the beginning, middle and end of the line, but rather with a test made by intercepting the vision at a corresponding point in the line. The comparative numbers thus obtained were 5.4 and 5.1; that is, after the long pause, which a period allows, the eye lengthens its lead of the voice.

Of *subjective* influences determining the distance between eye and voice obviously one is the familiarity of the reader with the thought and language. Rate of reading is also closely related, especially in two of its factors, eye-mindedness and quickness of visual perception. The correlation is given below:

---

[1] Legibility has been carefully studied by Sanford: *Relative Legibility of the Small Letters, Amer. Jour. Psych.*, I., p. 402.

TABLE XVII.

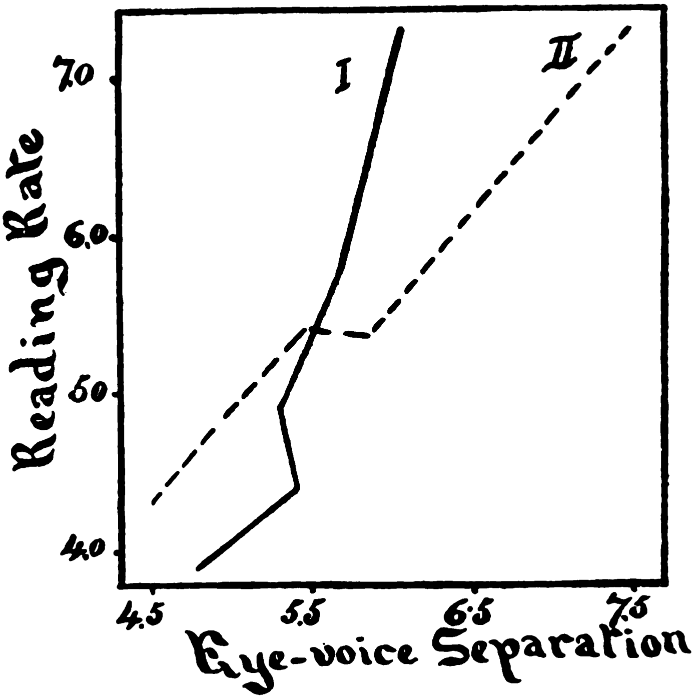
EYE-VOICE SEPARATION DETERMINED BY READING RATE (I.).

Class	XX	X	M	A	AA
Rate	3.9	4.4	4.9	5.8	7.3
Eye voice	4.8	5.4	5.3	5.6	6.0

RATE BY EYE-VOICE SEPARATION (II.).

Eye-voice	4.5	5.0	5.4	5.8	7.5
Rate	4.32	4.96	5.41	5.33	7.24

Curve 17.



If, as is thus shown by the table and Curve 17, those who are rapid silent readers read farthest ahead of the voice in reading aloud, and if a certain considerable distance between eye and voice is a condition of intelligent and intelligible reading, it follows that here again, as in silent reading, rapidity is an advantage.

It is perhaps worth considering whether we mean, when we say the eye is any given number of words in advance of the voice, that the understanding also is, or whether it occupies a place intermediate between eye and voice. Does comprehension keep pace with visual perception, or does it lag behind? There is also another consideration, namely, that visual perception itself has not advanced so far as the tests seem to indicate. As in the experiments with the exposure apparatus, retinal images may be formed at the very moment when the view is intercepted. Some time is required (as physiology tells us) for the conveyance of this impression to the brain; further time is spent (psycho-physics adds) in the conversion of the impression into a sensation; and still an additional interval (psychology shows), though this is perhaps inappreciable, before the sensation becomes a perception. These physiological and psychical processes may take place after the physical stimulus has been removed. But aside from this, and after we have reached *word-perception*, does the understanding of the *meaning* of the groups of words advance along with this? The question must probably be answered in the negative, though the number of experiments made were too few to justify definite conclusions. The method of testing consisted in giving the subjects such sentences to read as "A large bass was caught in the river," "Does in the park are not so fleet as when running wild," "When the child saw the tear in her sister's dress she was very sorry," each containing a word which would probably be mispronounced unless its true meaning in the sentence was understood before the word was spoken. This misapprehension would probably take place unless the understanding of the words was in advance of their utterance by two or three words.

§ VII. RÉSUMÉ.

1. Colors are more easily perceived than geometrical forms, isolated words than colors, and words in construction than disconnected words.

2. The visual type of persons are slightly more rapid readers than the auditory type.

3. Rapid readers not only do their work in less time, but do

superior work. They retain more of the substance of what is read or heard than do slow readers.

4. Lip-movement is a serious hindrance to speed of reading, and consequently to intelligence of reading. The disadvantage extends also to reading aloud.

5. Apart from external conditions, such as time of day, physical fatigue, etc., some of the influences contributing to rapidity of reading are largely physiological, as visual perception; others are of mental endowment, as alertness of mind; or of training, such as concentration of mind; still others are matters of mental equipment rather than intellectual ability; for instance, extent of reading and scholarly attainment. The order of importance of these is probably as follows: visual perception; practice, as determined by amount of reading from childhood onward; power of concentration; mental alertness, estimated by rapidity of original composition; scholarly ability, as decided by college records.

Some of these factors are doubtless not ultimate; differences in quickness of mental operation, for example, have, in all probability, corresponding differences in the functional activity of brain cells.

It might be added as a particular verification of these general conclusions that by far the most rapid reader of all those tested is a young woman whose extent of reading is exceptionally broad, and who possesses a strong tendency toward eye-mindedness, a marked power of mental concentration, and intellectual ability of a high order—all of which have been found to be positive factors contributing to rapidity of reading. She is a brilliant conversationist as well, and in writing cannot make her pen keep pace with her thoughts, thus showing an unusual quickness of mind.

APPENDIX.—PERSONAL SHEET.

1. Do you consider yourself a very slow, slow, medium, rapid or very rapid reader?

2. Do you enjoy and profit by having some one read aloud to you?

3. Do you read aloud to yourself? Does this aid you in comprehending and retaining?

4. Do you habitually move your lips or tend to do so when you read? And if not habitually, do you ever do so? Under what circumstances?

5. What processes accompany your reading? In particular, do you imagine the sounds of the words read? Do you imagine yourself speaking the words? Is the whole process a purely visual one of reading the words, and nothing more? Answer as fully as possible.

6. Do you recall quotations readily? Do you recall and commit verse readily? Would your regard your memory for what you have read as very poor, poor, medium, good, or very good? Answer for different kinds of reading if possible.

7. Are you readily disturbed when reading by talking or other noise going on in the room?

8. Do you comprehend and retain better the substance of a lecture, or the same matter read by you once only, in the form of an article? Estimate the extent of the difference.

9. Would you regard the amount of your reading as very limited, limited, medium, extended or very extended? In what lines has your reading been most extensive?

10. Do you write very slowly, slowly, medium, rapidly or very rapidly? Answer, first, regarding mere mechanical writing, as from dictation or copy; secondly, regarding composition, as in writing a letter or an essay.

11. Add any special information bearing upon the questions above, that pertains to your reading and other habits.

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THE FLUCTUATION OF ATTENTION.[1]

PART I. INTRODUCTORY.

Attention is generally conceded to be the *terra incognita* of psychology. It is, however, a subject which has come prominently to the front of late, and has become a topic of some experimentation. So far, however, little more has been done than to present the problem, and to show some of the difficulties in the way of its solution.[2] The present study, however, was not begun so much with the hope of solving the difficulties which attend the understanding of attention, as with the hope of giving some light upon a practical problem of ethics, which, no doubt, in some form or other, confronts every intelligent human being.

There are probably few who have not observed in themselves or others an occasional change of feeling for another person, a certain occupation or amusement. It may have been but a passing wave of dislike, or a short period of irresponsiveness; while it may have been an intense feeling of revulsion. Usually this comes for no adequate reason, or no reason at all, and disappears as unceremoniously as it came, often leaving in its wake a heightened feeling of appreciation, its polar opposite. This aversion may last but a few seconds or hours, or it may continue for months or even years, and then perhaps wear away so gradually that its disappearance is made unconscious. Perhaps the ways in which this inconsistency appears to us the most strikingly are in the fluctuations of religious feeling as shown by occasional 'backslidings;' the periodic inability to deny some strong appetite, as it often is with the inebriate who is trying to reform; and the changes of attitude in the individual's social relations, as evidenced by the great number of divorces and the complications of social etiquette. If, however, we look more closely, we find that a marked similarity exists between these more striking forms of changeability and other mental phenomena. The enjoyment of variety in the styles of dress and architecture, music, the revulsion at being bored, the monotony of routine, might be taken with a host of other common experiences to illustrate this same principle; but enough has already been said to make clear the central idea here implied, that the power to work along the line of a certain mental activity tends to be intermittent rather than continuous.

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[1] The laboratory work here described was done at Clark University. I wish to thank the members of the laboratory for their hearty coöperation, and to express my indebtedness to Dr. Hall for his suggestions and encouragement, and to Dr. Sanford for his constant assistance and supervision.

[2] For recent experimental work on attention see 'Die Aufmerksamkeit und die Funktion der Sinnesorgane,' by W. Heinrich, *Zeitsch. für Psychol.*, Vol. IX., Nos. 5 and 6, pp. 343-388.

'Attention: Experimental and Critical,' by Frank Drew, *Am. Journal of Psychol.*, July, 1896.

'Attention and Distraction,' by Alice J. Hamlin, *Am. Journal of Psychol.*, October, 1896.

The primary problem presented by this condition of things is, How far are we responsible for this inconstancy and the conduct which naturally follows from it?

By looking closely into these instances of change in the mental attitude we see that we have to deal with conditions of attention. The prevalence in the mind of an idea is of itself enough to control conduct. In speaking of the effect of an idea upon action, Professor James says: "Let it once so dominate, let no other ideas succeed in displacing it, and whatever motor effects belong to it by nature will inevitably occur—its impulsion, in short, will be given to boot, and will manifest itself as a matter of course. This is what we have seen in instinct, in emotion, in common ideo-motor action, in hypnotic suggestion, in morbid impulsion, and in *voluntas invita*, the impelling idea is simply the one which possesses the attention."[1] We may ask, Is it simply moral perversity which prevents the child from studying his lessons constantly, or are there natural and sufficient reasons which make it impossible for him to do so? Does it show insincerity when the clergyman leaves his parish and seeks recreation where demands of a religious nature will not be made upon him? And, finally, does a violent or passionate act

[1] James's Psychology, Vol. II., p. 559.

necessarily portray a villain, or may it simply indicate the exaggerated intensity of a normal functioning ? These questions are all evidently included in the problem which it is the main object of this research to help solve, namely, What determines the time at which and time during which a given idea or set of ideas shall have a predominating influence upon the mind ; and what determines the intensity of that domination ? In short, what determines the direction and intensity of attention?

Even with our subject thus limited, it is evident that an exhaustive treatment, such as would follow it into all of its ramifications, would require nothing less than a working over of the whole field of psychology. Artificial limits will therefore have to be placed, and omissions made necessary which may seem to deprive this treatment of its due symmetry and completeness.

While it may not be found that the following of any rigid classification of work will be of advantage in the present paper, we can perhaps best locate ourselves in this field of inquiry by dividing the work in a more or less formal way.

It is evident that there are two grand divisions to our subject as a whole :

First, the study of the conditions favorable to a change in the direction of attention.

Second, the study of the conditions favorable to the retention of one direction of attention.

These conditions may be divided into those that are (1) subjective, or those referring to the particular state of the person's mind; and those that are (2) objective, having to do with objective conditions and the nature of the object of attention.

We, therefore, have :

1. The subjective conditions for the change in the direction of attention.
2. The subjective conditions for the retention of the direction of attention.
3. The objective conditions for the change in the direction of attention.
4. The objective conditions for the retention of the direction of attention.

In the present paper I shall be able to deal at length with only the first two of these divisions.

We need not look far to find a close analogy to these phenomena of intermittence in the fatigue and rest which we all undergo each day. At night the limbs feel heavy, a general uncomfortable feeling pervades the body, and the mind acts slowly and with frequent distractions. A good night's rest, however, seldom fails to relieve these symptoms of fatigue and brings with it the buoyancy and vigor always associated with morning hours.

Another fact that indicates mental distraction to be a result of mental exhaustion is that young children are incapable alike of continued mental or physical application, showing inattention after an attempt at the one, and the familiar signs of bodily fatigue after the other. Dr. Hürliman states that a child of four cannot keep a uniform upright position for more than five minutes.[1] Continued mental effort is found to be correspondingly difficult. In connection with certain researches of Dr. G. Stanley Hall, I have watched children in the kindergarten when set at making short, vertical lines (soldiers) between parallel horizontal lines, upon paper. Children between four and five years showed evident signs of fatigue within ten minutes of the time of starting. When the experiment was conducted longer these signs became more pronounced. The work would be momentarily dropped to look at one's neighbor's work, or at some object at hand that was more entertaining. When urged to continue his work, the child would make a few lines and then relax again into his general observations. Signs of physical fatigue were also marked. The posture would be frequently changed, the pencil changed from one hand to the other, the head laid upon the arm, and the feet frequently shuffled.

Since inattention and muscular fatigue are conditions that seem so nearly parallel, let us examine more closely the principles that control the latter. It is well known that the continued working of a muscle will result in the inability to work through fatigue. A heavy weight can be held at arm's length but a moment. The heart rests after each contraction; and the process of respiration consists of a series of muscular contractions and relaxations. This fatigue is perhaps best studied by means of the ergograph, the instrument used by W. P. Lombard and Professor Mosso. By means of this instrument a single muscle can be worked until exhausted, and the amount of work done in doing this accurately estimated. By placing the right palm flat upon the table and moving the index finger to the left, the muscle is used which was employed in the following demonstration. A cord, attached to the outer end of a thimble fitting tightly on the end of the index finger, runs to the right, passes over a pulley, and is attached to any weight desired. A pen moving horizontally and attached between the finger and the pulley marks upon the revolving drum of a kymograph, thus registering the amount of each contraction or the height to which the weight is lifted. The following cuts on page 6 are from records taken from myself.[1]

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[1] *Zeitschrift für Schulgesundheitspflege*, No. 6, 1892, p. 266.

From *a* to *b*, Fig. 1, are shown the contractions of the muscle above indicated under a weight of 846 gr. The contractions were made once a second. It is seen that the degree of the contractions grows less until *b* is reached, when the power of moving the weight was lost.

Fig. 2 shows a similar record when the weight had been reduced to 706 gr. It is seen that the contractions were more ample and continued longer, thus making the amount of work done approximately the same.

Fig. 3 is a record taken when the weight was 846 gr. (the same as in Fig. 1), but the contractions came once in two seconds instead of once a second. Here we have 35 distinct contractions as against 21 in Fig. 1, and, besides, the amplitude of the contraction is considerably greater. Many other records might be given to illustrate this principle: that the harder the work, the quicker is the exhaustion.

The phase brought in by Fig. 3 is evidently explained by the constant process of recuperation which seems to take place. This is also illustrated by the portion *c d* of Fig. 1. After the muscle had became so exhausted that it became impossible to raise the weight, a rest of eight seconds represented by the line *b c* was allowed in which no effort was made. Then the contractions shown by *c d* were made. This shows that the process of recuperation must have gone on rapidly during the rest.

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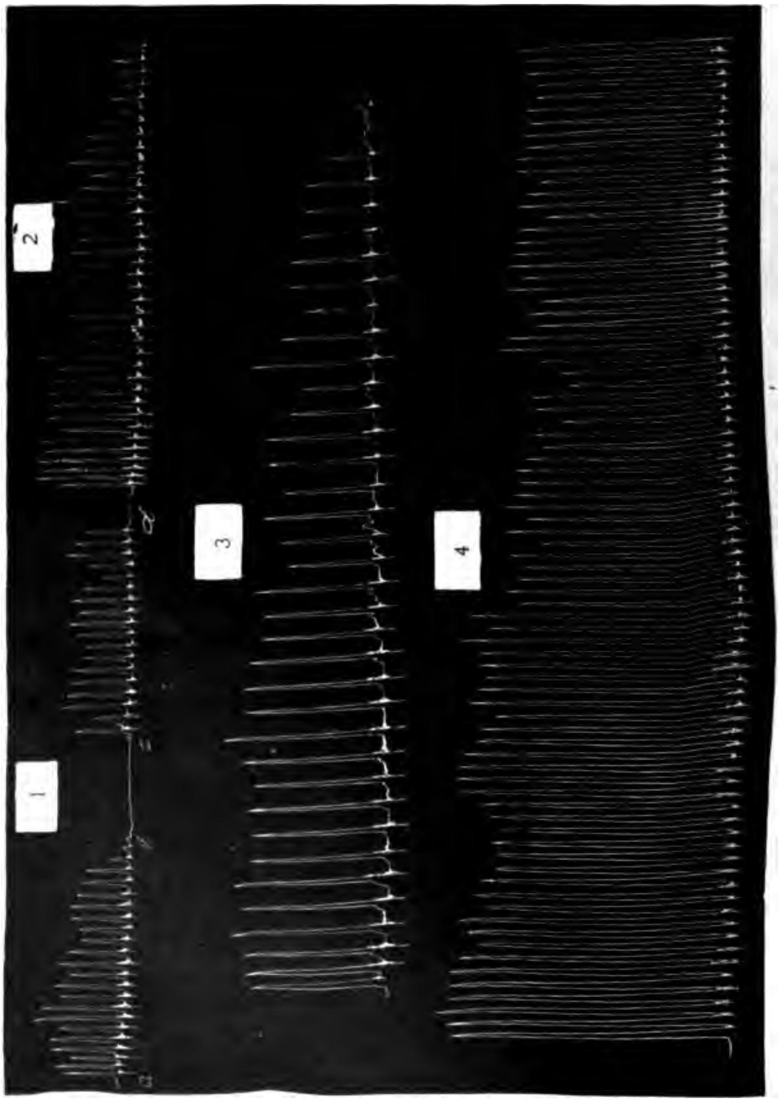
[1] See Angelo Mosso, 'Les lois de la fatigue étudiées dans des muscles de l'homme.' *Travaux de Lab. de Physiol. de Turin*, 1889, pp. 149–212.



Figs. 1, 2, 3, and 4.

Fig. 4 shows this in a no less striking manner. Here the contractions were made once a second, while the weight was but 445 gr. The contractions are at first markedly ample and lessen only slightly when the end is reached. The actual experiment was continued for 166 contractions more without any distinct decrease in the amplitude. This shows that the rate of working represented by the 'curve,' after this slight decrease at the beginning, was balanced by the rate of recuperation.

Exact mathematical measurements might easily be made showing the equivalent of this work in dynes, and the value of the energy generated. So many varying physical and mental factors influence this, however, that it would be difficult to estimate them for the purpose of foretelling kinetic results. Yet enough has been given to make it plain that the rate of muscular fatigue indicates the ratio between the rate with which energy is supplied to the muscle and the rate with which it is drafted off.

The following axioms are directly or indirectly an evident result of these data:

1. The more intense the activity, the quicker is the fatigue.
2. The longer the activity the greater is the degree of fatigue, if the demand for energy is greater than the supply.
3. The greater the degree of exhaustion, the slower must the rate of working become.
4. The more complete the rest, the more rapid is the recuperation.
5. The longer the rest the greater the recuperation.
6. The greater the degree of recuperation, the greater is the rapidity of work made possible.

Now Professor Lombard[1] seems to show that this fatigue is due almost entirely to the exhaustion of the nerve cells which innervate the muscle, from the fact that when the power of voluntary contraction is lost by fatigue, contractions may be produced by an electric current administered to the nerve. What we have been studying, therefore, is evidently the laws of neural fatigue.

Before proceeding to the more purely mental aspect of our problem, I wish to note briefly the results of a previous research made with Professor Münsterberg at the Harvard Laboratory, the account of which has been in part published.[2] From this work it was made evident that what has been called the fluctuation of attention, as when a slight visual impression alternately comes and goes, is not due to a change in the direction of attention but to a fatiguing of the sense organ, or at least to a change in the nervous mechanism less central than the seat of consciousness. One of the strongest reasons for this is that when the ocular impression is no longer noticeable, the object can be visualized mentally, thus showing that the attention still retains it previous direction. In fact if the attention had changed its direction when the visual object became invisible, the moment o its disappearance would have been unknown, and the idea o the object would not have returned until the object reappeared no distinction between the visual image and the sensation bein possible. The same was found to be true of sensations of touc and of temperature. It seems safe to say, therefore, that th sense organs act independently of the attention.

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[1] *Am. Journal of Psychology*, Vol. III., p. 24.

[2] Report from the Harvard Psychological Laboratory, *Psychological Review*, January, 1896.

By taking the time of consecutive disappearances and appearances in long series of these sensations, and varying th intensities of the stimuli, it was found that for ocular sensation the larger and the lighter the object (on a black background the greater became the time it was seen as compared with th time it was not seen.

For sensations of temperature it was also found that the mor intense the sensation the longer was the sensation felt in comparison with the time it was not felt. For auditory sensation the same seemed true, though conditions prevented a satisfactor trial.

With sensations of touch the reverse was true, though thi may be accounted for by the fact that, as touch came to be mor and more distinct pressure, it prevented, to an increasing exten the circulation necessary for sustaining the sense organ.

Here, then, it will be observed, is a set of data exactly opposed, as it would appear, to the axioms just drawn up fo neural activity in the case of muscular action.

The fact that what has been commonly called the fluctuatio of attention is not a central phenomenon, does not, howeve prevent its real existence at the centre of consciousness. Although the mental image of a visual object may survive th ocular image by a considerable time, usually completely bridging over the absence of the latter, the mental image itself is subject to alternate disappearances and appearances. Recent work of Dr. E. H. Lindley, to be referred to later, gives valuable data upon this point. Let any one visualize (with eyes closed if more convenient) a simple object like a red disc the size of a penny. At first it is very distinct. The color is clear and natural, and slight blemishes and irregularities, usually found in such a surface, are not left out. After a moment the color fades, and the disc disappears, in spite of the effort to retain it. It shortly reappears, however, with a distinctness comparable to that of its first appearance. This experiment can be continued indefinitely.

A similar experience is had when one tries to recall a word or name once forgotten. All irrelevant ideas are inhibited, and the attention held as if by some vague notion of its whereabouts over a blank or cavity which corresponds in some general way to the features of the word desired. Suddenly, if the effort is successful, it pops up as if by its own accord. Often this process is assisted by some cue which acts through some path of association. But even then there would seem to be some principle of spontaneity acting in the ideational centres which is not fully under the control of volition.

PART II. EXPERIMENTAL.

Experiment A.

Since it is evident that the inability to continue in one line of mental work may result from fatigue, I wished to arrange some kind of mental work which would exercise but a narrow range of activity, and yet keep that activity at its highest degree of intensity. The object was to study the effect of fatigue upon the attention involved.

A tape one-half inch wide and eight feet long was prepared, having typewritten capitals on one side. The letters were separated by a space equal to that used between adjoining words; for one half of the tape no one letter was followed twice by the same letter; the other half was a repetition of this. The tape was joined at the ends and made to revolve in belt fashion horizontally by being supported by a rimmed truck at one end, and the drum of a kymograph at the other, the latter being used as a motor. At one end of the tape was placed a screen with an opening just large enough to allow the subject to see one letter at a time as it passed. The experimenter, placed at the other end, could read the letters from the other half of the tape in the same order as the subject, but without being confined to a small screen-opening.

The tape was set revolving at the fastest rate at which the subject could read the letters aloud. Whenever a mistake or omission occurred, the experimenter recorded the same with an electric key upon a smoked drum. The subject was also provided with a key which recorded his conscious errors, and was practiced in its use, to make the recording as automatic as possible. It was soon found that the subject's mistakes were conscious with hardly an exception, and that his record of errors was, if anything, more correct than the experimenter's.

Each experiment was conducted during a time of from ten to forty minutes, so that fatigue was likely to be involved to a considerable extent. The rate varied for different subjects, and at different times for the same subject, from the passing of two to the passing of four letters by the opening of the screen in a second; but the rate during one experiment remained constant.

The kymograph records of these experiments show: First, that the errors increase towards the end of the experiments; especially in the longer ones; and, second, a marked tendency for the errors to come in periodic groups. A group of errors unquestionably means that the faculties exercised by the subject are acting less energetically and are, therefore, having a period of comparative rest; and this rest is shown by the renewed activity which followed each group of errors.

These results are obviously in accordance with the laws of muscular fatigue as indicated above, where it was seen that an expense of energy greater than the supply was followed by the inability to work, and that this in turn was followed by a period of activity.[1]

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[1] See Lombard, loc. cit.

Let us now turn to the subjective experiences accompanying this.

When the experiment was continued long enough to make the errors much more frequent toward the end, a feeling of fatigue became prominent. At such times several letters would be missed at once instead of one at a time, as it more frequently was before distinct fatigue set in. The subject would know perfectly well that several letters had escaped him, but the power to bring the mind back sharply to its work was wanting.

The question may be asked as to how far this fatigue was involved in the vocal muscles and eye adjustments. But one subject (Sh.) felt much fatigue in the muscles of the throat. This was not extreme enough, however, to interfere materially with speech. No fatigue was felt in the eye muscles.

There was often a tendency to get behind in the perception and pronunciation of the letters. Sometimes each of several letters would get by a little farther before pronounced until one would have to be skipped in order to catch up. This in itself would produce a comparatively regular periodicity in the errors even if there were no periodic fatiguing. The kymograph records show, however, that the errors did not often come regularly enough to be accounted for in this way, and that when the errors did come with great regularity they were so far apart as to be separated by eighty to a hundred letters. A gradual retardation, for so long a time as this, however, was not experienced by the subjects. The subjects' accounts of this show also that the tendency to get behind was much more frequent than the actual omission, and that a special effort made on that account often resulted in catching up again.

This brings us to perhaps the most interesting phase of the experiment, the distracting effect of extraneous ideas. There was no rate of speed that could be given the tape that would allow the subject to keep up and yet be an effective bar for any length of time against all foreign ideas. Yet it was found that, so long as the subject could keep up, the faster the rate the greater was the distraction of other ideas; and that ideas entirely disconnected from the work were more distracting than those immediately connected with it. Thus, the idea of getting behind, although different from that involved in the work itself, often resulted in catching up, while the knowing of one's crossing the floor caused an omission.

The occasional flitting in of other ideas, though perhaps better called vague feelings than ideas, when the subject was straining every nerve to keep up, and yet, in some cases, without any apparent disturbance, suggests the interesting question as to where the attention was at the instant that this idea took its prominence in the mind. The sense organs were adjusted to the letters on the tape, and the muscles were working to favor that adjustment and to express the appropriate motor response. According to Ribot, therefore, the attention was on the tape. Professor James might consider it to be on the transient idea—we will say the idea of going too slow—although he would seem obliged to abandon the *sense organ adjustment* part of his theory. True, it might be said that the sense organ adjustment would be the same for the supposed idea and for the tape. But if sense organ adjustment may be the same for several different ideas, then it is evident that this ceases to be a part of attention which articulates itself characteristically for each idea, but only a rough and general attitude accompanying general classes of attentive states.

I can hardly see how the *muscle-sensation* part of Wundt's theory comes in in this case, though the presence of the feeling which precedes the ideational change and the growing clearness of the idea would seem to commit him in favor of the intruding ideas. And, since the subject might be supposed to have an *interest* in pronouncing all the letters which passed, we could doubtless place Stumpf with Wundt.

The distracting effect of many ideas suggested the application of some painful stimulation to the subject, while reading the letters at his fastest rate. Accordingly, a constantly increasing alternating current of electricity was passed through the hand of two of the four subjects in this experiment, Sh. and myself. When the current had reached to but a slight intensity no effect was noticed, except, perhaps, a slight distraction. But when a distinctly painful intensity had been reached, it seemed to serve as a prop to the attention and caused the mind to act with more than usual clearness and alacrity. When, however, a much stronger intensity had been reached, the attention was diverted largely to it, causing long omissions in the reading. It was found that the reading of a story to Hh. had the same effect as the moderately painful current had upon Sh. and myself, and that the subject was afterwards able to give a good account of what had been read. A careful record taken from this subject with and without the reading showed that there were actually fewer mistakes with the reading. The opposite, however, was true with Sh. and myself. With all the subjects there was a strong tendency for the naming of the letters gradually to become automatic.

Experiment B.

In the last experiment the amount of work to be done remained constant, while the periods of relaxation were recorded by means of the errors. In Experiment B, I wished to measure the degree of relaxation primarily by means of the changed rate of working.

To do this I arranged columns of figures for adding, upon three large sheets of paper, eight and one-half by eleven inches. There were fifteen columns on each sheet, arranged in groups of three columns each, with twenty-seven numbers in each column, each number composed of one digit, zero being omitted. Each digit from 1 to 9 was used approximately the same number of times, though not so strictly as to exclude a certain amount of variety in the sums of the columns. A variety in the order of the figures was also introduced. These sheets were distinctly printed by means of the mimeograph, so that each subject could have his own set. The columns were distinguished for purposes of reference by the use of letters.

The experiment was conducted as follows: The sheets were arranged in order upon a table before the subject, the experimenter gave the signal for starting, and took the time required for adding each of the forty-five columns in turn, the subject constantly adding at the top of his speed. The signal for the finishing of each column was given by the subject's pronouncing aloud the sum at the head of each (the subject adding upward), and the experimenter taking the time to fifths of seconds from a stop-watch, and also recording the sum given.

Each of the four subjects in this experiment added these fifteen groups of columns fifteen times, beginning the first time with the first group; the second time with the second group, and ending with the first; the third time beginning with the third group and ending with the second, and so on. The time required for the experiment varied with the subject and with the amount of practice. With one it took over three-quarters of an hour the first time, but with another less than twenty-five minutes.

Let us now turn to the method of working up the results.

The time taken for the addition of each of the columns was first arranged in forty-five columns, each column having fifteen time-measurements. Those in the first column of time-measurements showed the time for adding the first column of figures at each trial, in whatever part of the sheets the trial began. The second column of time-measurements showed the time required for adding the second column added at each trial, and so on. These columns of time-measurements were added separately and the average time-measurements found for each column. Each three of the columns was then averaged in turn, thus giving fifteen averages corresponding to the fifteen groups of columns added in the experiment. The effect in this result of beginning at different groups at the different trials is evidently to obviate the error resulting from one group being harder to add than another, thus keeping this objective condition constant. What these fifteen averages indicate is, therefore, the changes in the subjective conditions, due, we will suppose, to fatigue. This, however, is not strictly true, since the experiment was sometimes interfered with by more or less distant noises which frequently had the effect of exaggerating subjective tendencies. The solid curves[1] in Fig. 5 are plotted from these averages taken from the four subjects. There are a few points common to these curves which it may be well to note. There is at first a rapid fatiguing shown by the increase of time which the second group took over the first. Next, a more or less marked acceleration, due, evidently, to a return of vigor, makes a new tack in the curve. This is followed by a variety of ups and downs to the end, towards which, in all but one case, there has been a gradual slowing up. The subject's reason for the acceleration here was the anticipation of the end. The other subjects were not conscious of this.

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[1] The figures at the left of the curves are a scale of the time in seconds, thus showing the rate of adding for each part of the sheets. The figures at the left of the dotted lines give a scale of mistakes.

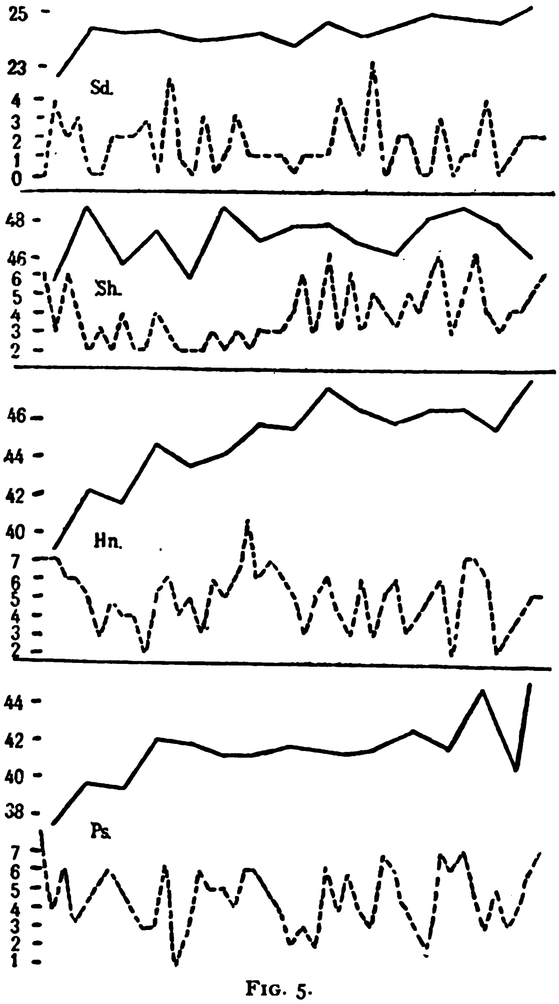

FIG. 5.

These curves, however, do not give a correct impression of the variations in rate which actually occurred during one trial. This is because the process of averaging often offset a rapid addition in one place by a slow one in the corresponding place of another trial; and besides grouped too many columns together. To obviate this difficulty, a different system of tabulation was required. In this system the time taken to add a certain column on the sheets during the fifteen trials was kept in a column by itself, thus the column marked *A* on the tabulation sheet gave the time-measurements taken from adding the *A* column on the sheets of the experiment for the different trials. The record of each subject was kept separate as in the last case. Here the rotating method followed in the different trials served to distribute the general fatigue effect equally among the tabulation columns, thus making necessary another method than in the last system for the elimination of differences of difficulty in the addition columns. To do this, the tabulation columns were added and the average found for each column. A grand average was then taken of these averages. This final average was subtracted from each of the column averages, thus leaving for each column the difference, sometimes preceded by a plus and sometimes by a minus sign. When this difference was properly combined with any time-measurement of the column to which it belonged, it is evident that it effected a correction for the comparative ease or difficulty which the subject found in adding the corresponding column for that trial in the experiment sheets.

Thus, suppose the average for the tabulation column *A* to be 42.6 sec. and the grand average 41.3 sec. The difference, when 41.3 sec. is subtracted, is + 1.3 sec. This shows that the time for adding column *A* on the experiment sheets averaged 1.3 sec. more than the total average, or was by that much more difficult to add than the average. This amount then must be subtracted from each of the time-measurements in tabulation column *A* in order to apply the needed correction. Each measurement of the fifteen trials was corrected according to this method for all of the four subjects, and each trial was platted into a curve.

The following illustrative columns of figures show a comparison in seconds between the time-measurements before and after correction of the first and last trials of Ps.

As before stated, the sums were taken as pronounced by the subject in the experiment and the errors in addition thus kept account of. These were represented with the platted curves by placing a cross along a horizontal line above each curve at the point at which the mistake occurred. These mistakes were combined in the dotted curves, in Fig. 5, which show in what parts of the trials the mistakes were most frequent.

TABLE I.

FIRST TRIAL.						LAST TRIAL.					
Before Correction.	After Correction.	Before Correction.	After Correction.	Before Correction.	After Correction.	Before Correction.	After Correction.	Before Correction.	After Correction.	Before Correction.	After Correction.
39.0	34.4	57.8	49.5	59.0	60.2	35.0	30.4	46.0	37.7	40.4	41.6
51.0	50.7	44.2	39.8	49.6	51.0	31.4	31.1	40.4	36.0	45.4	46.8
39.4	45.3	49.0	52.4	45.0	47.9	31.2	37.1	34.6	38.0	35.4	38.3
61.4	61.1	31.0	55.3	62.0	59.7	32.8	33.1	43.0	47.3	40.8	38.5
50.4	48.0	49.0	52.9	53.0	52.7	31.2	28.8	28.4	32.3	31.0	30.7
43.0	43.4	58.8	55.3	50.0	49.4	25.0	25.4	22.0	18.5	31.0	30.4
46.4	47.0	43.0	44.0	33.0	40.9	37.0	37.6	33.4	34.4	30.6	38.5
77.4	74.7	65.0	60.7	43.4	46.9	33.2	30.5	35.6	31.3	49.6	53.1
51.2	51.7	73.0	69.6	60.0	61.5	37.0	37.5	29.4	26.0	38.0	39.5
48.0	51.5	62.0	62.3	59.0	54.9	33.0	36.5	36.0	36.3	22.4	18.3
42.4	46.9	59.4	60.0	49.8	55.7	38.0	42.5	30.8	31.4	28.4	22.5
59.6	63.6	47.0	49.6	51.6	52.7	29.0	33.0	53.0	55.6	23.8	24.9
50.4	47.1	56.0	53.9	43.2	40.2	31.0	27.7	34.0	31.9	33.4	30.4
65.0	68.1	65.4	63.3	53.0	56.7	38.0	41.1	34.0	31.9	49.0	45.3
51.0	55.8	45.0	46.4	44.6	42.6	31.0	35.8	26.0	27.4	51.2	49.2

Even with the system of correction given above, a slight error arises from the fact that the practice shortened the time for the latter trials; so that while the amount added or subtracted for the needed correction was estimated for the average time required for a trial, it would be somewhat too small for the longer first trials, and somewhat too great for the shorter last trials. But as the features of each curve are pronounced, this correction would not materially affect them, this correctio made, even in extreme cases not often exceeding a second.

Two typical curves illustrating the result from all the jects are given in Fig. 6 from Sh. The first represents the

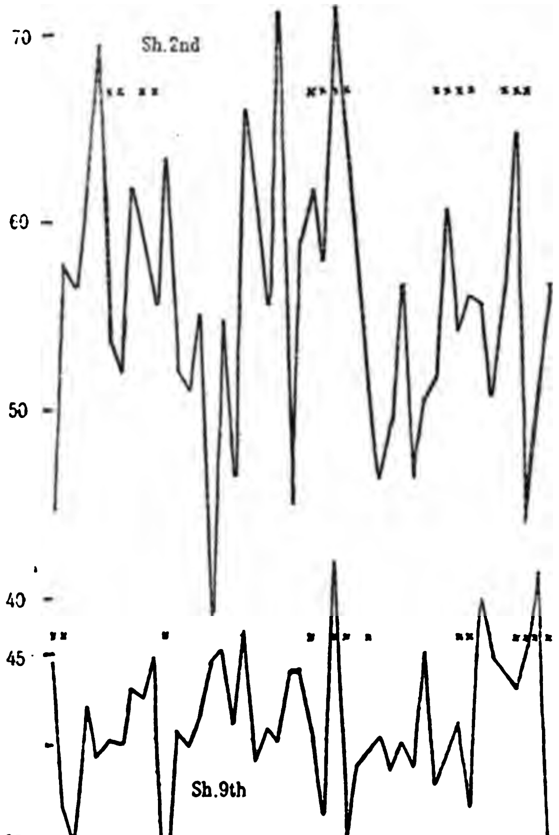

FIG. 6.

ond trial and the second the ninth. It is at once seen that t are great variations in the rate of adding, it being almost do for some columns what it is for others, and that these extr are less marked in the later trials than in the first. In the trials there are almost immediate transitions from the faste the slowest rate, while in the later ones the medium rates occupy a much greater proportion of the curves. It almost never occurs that two successive points in a curve represent an extreme in the same direction. When several columns are added at a fairly constant rate they are usually followed by quickly alternating extremes. A general acceleration or retardation in one part of a curve is followed by a similar tendency of the opposite kind. A gradual tendency towards a fast or slow rate is often offset by one or two extremes in the opposite direction.

If these features are compared with the principles of exhaustion and recuperation found in muscular fatigue, it will be seen that they are covered by axioms 1, 3, 4 and 6 of the ergograph experiments, page 7. In short, there appears to have been a fairly constant and limited supply of energy that was drafted off, sometimes faster and sometimes slower, by means of this work. Perhaps the only feature found here that is not seen readily in the more common phenomena of fatigue is the frequent substitution of rapidly alternating extremes of function for a gradual loss of power. But this can be readily accounted for by the almost complete control offered by the experiment over the supply of energy used in the work, and by the nutritive conditions which allow rapid recuperation; more of which, later.

Careful examination shows no constant relation between the rate of adding and the mistakes. The dotted curves (Fig. 5) show also that there is no constant increase of mistakes for all of the subjects in any part of the trials. There is in each trial, however, a frequent tendency for the mistakes to be grouped.

Let us now turn to the subjective experiences.

The first trials without exception were very fatiguing, so much so that the work was dreaded. Then, after practice, it became gradually less so until it seemed but ordinary though active exercise.

As the methods of the experiment served to keep the objective conditions fairly constant, the variations in the results may be taken to indicate the subjective variations due to the work. So far as possible the subjective conditions were kept constant. With the exception of Ps., the same hour of the day was used except for one change of time in each of the other subjects. This change coincided with no marked change in the record except in the case of Hn. But if the curves in Fig. 7, which show the time effect of practice, are studied, the daily condition of the subjects — with the exception of Sd. — is seen to have varied considerably, as shown by the irregularity of the curves.[1] The unusually long times taken by some of the trials could not always be accounted for; when they could be, a general lazy feeling, the intrusion of extraneous ideas, and a feeling of fatigue from the work of the day figured most prominently. The shortening of the time, beside the effect of practice, seemed due to an unusual vigor. It is evident that better methods for controlling subjective conditions must be developed before anything like exact work can be done in experiments of this kind.

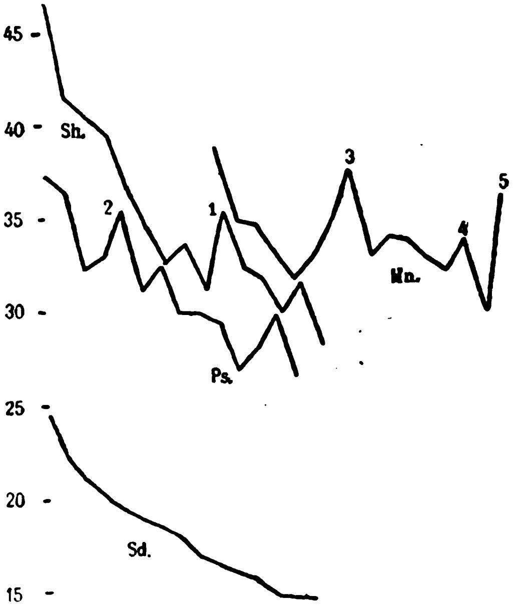

Fig. 7.

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[1] The curves proceed from left to right and show the time of adding for the successive trials. The scale to the left denotes the time in minutes. 1. Not feeling well. 2. Unread letter in pocket. 3. First change from 3 P. M. to 9 A. M. 4. Subject felt lazy. 5. Fatigue with adding, to start with.

In turning to the conscious accompaniments of the periods of fatigue as shown by the curves for each trial, we find a variety of experiences. The very long periods shown by the curves (see Fig. 6 for illustration) may be caused in any one of three ways: first, by a suspension of the mental process in which the mind waits in a state of vacancy for the mental images used in adding to arise; second, by the displacement of the mental process by extraneous ideas, sometimes brought in by interrupting sensations; and third, by a confusion in the process of adding, frequently resulting in a mistake.

These disturbances would sometimes become prominent near the end of the trial, thus indicating a general fatigue, but more frequently they became prominent by the time the middle was reached and often before.

The suspension of mental activity is the same as that found in the last experiment. Voluntary effort is of no use in starting the process. There is a vacant feeling of suspense, and the subject is obliged to await the return of the mental images. This state may occur at almost any time in the trial—though not often occurring among the first few columns—and may be induced by unusual effort (axiom 1, p. 7). This vacancy may be filled by other ideas, and the subject brought to inhibit them in favor of the adding by means of the sensory and mechanical attitude which serves as a reminder. Often the ideas that fill up a vacancy in this way are of the most remote kind, and not connected by any conscious process of association with the ideas that preceded them. When some sensory stimulation interfered with the work, or when there was some interesting object in the background of consciousness, the degree of fatigue necessary for the rise of the irrelevant idea was much less; then it was accompanied with the feeling of intrusion. When the sensory stimulation was great, or when the idea in the background was of special interest, these conditions would evidently be of more significance in deciding what idea shall be uppermost in consciousness than the slight fatigue from adding.

It is thus seen how the first conscious reason for delay in adding gradually merges into the second, and this seems to merge equally well into the third, the confusion in adding which results in a mistake. When, in adding, an extraneous idea comes to predominate in the mind, the image of the sum goes out. An ingenious way of retaining it comes consequently to be developed. The sum, up to the time of interruption, is automatically repeated with the vocal organs until the work is resumed, when the mental image of the sum is taken from the vocal expression. It is well known, however, that automatic processes are interfered with by vivid perceptual states. In this way the vocal repetition may be stopped by the intrusion, and the sum forgotten. A hesitation results from this, and the sum has to be guessed at or the column partly added over. The appearance of mistakes where the time for adding is short will be explained later. The grouping of mistakes (Fig. 6) seems largely due to the consciousness of having made a mistake, which acts as a distracting idea for the columns that immediately follow.

I believe the above description and explanation of the ways in which different ideas may displace each other apply as well to the experience of everyday life as to laboratory experiences. The familiar hesitating *-er* of a speaker, and ordinary absent-mindedness evidently find their explanation here, as also the rapid mind-wandering of the child.

Enough has already been said to indicate the tendency towards mental economy present in such a tedious process as adding. A close examination shows this to reach a high development in the variety of ways in which adding may be done. The object of this variety is probably to allow those centres employed in one way of adding to rest while those used in another way are exercised. At first the additions were made by combining one figure at a time to the sum immediately preceding it. This was when the process was slow and the rapid alterations in rate were very marked. After the first trial or two, two numbers would be combined, and later three before they were added to the preceding sum. One subject (Sd.) inhibited this tendency with great difficulty. Tens and twelves were favorite amounts to be made up and added in this way, though many others were used.

Another means of variety was the different ways two numbers could be thought of as combining. Suppose we have fifty-six and eight to add. I can remember that six and eight make fourteen, and thus arrive at sixty-four; I can say fifty-six and four are sixty, plus four make sixty-four; or, I can say fifty-six and ten are sixty-six, less two are sixty-four. These might be combined in various other ways, but these serve to illustrate. Still another means of variety was a resort to a kind of primitive adding by emphasizing alternate units in counting on a number. The emphasis divides the number into groups of units while a kind of parallel reckoning tells when the groups are exhausted. In making the large combinations mistakes sometimes occurred, which accounts for the presence of mistakes with quick additions.

The curves in Fig. 7, which show the effects of practice, indicate a quick falling off in the time required for adding. The same addition sheets were used later in other experiments by three of the subjects, Sd., Sh. and Hn. The increased practice thus brought in reduced the time for adding in each case to less than half the time required at first. We are now in a position to ask how this was brought about.

If we look at Fig. 6 we find two significant points: First, while the mean variation of time-measurements is much less in the later trial than in the former, yet the absolute time is so reduced that the relative changes from column to column in rate for each trial are approximately equal. Thus the fastest time in the earlier trial is 39 seconds, and the slowest 71.5 seconds; fastest in later trial, 30.6 seconds; slowest, 50.1 seconds; $39:71.5 = 30.6:50.1$ approximately. Second, in the later trial there is a greater proportion of medium rates. These observations suggest that the effect of practice did not wholly consist in the development of economic devices which would tend to make the rate constant, but also in the power of holding mental images and the facility of using them.

The subjective records show that, other things being equal, practice causes the sums to spring up automatically and with great rapidity in response to the mental images of the numbers.

When an interval of several days came between successive trials it was found that the facility in adding was to some degree lost. The sums would not spring up so readily, and there was a greater tendency to slower forms of adding. To test how far the particular combinations were remembered on account of the constant order of the figures, the addition sheets were reprinted with the order of each column exactly reversed. The following table allows a comparison of the time, in seconds, required for adding the inverted columns in three consecutive trials, with three trials of the normal order immediately preceding.

TABLE II.

	Sd.	Sh.	Hn.
Time before inverted order.	766.8 806.2 786.8	1633.4 1498.0 1478.4	1405.0 1345.0 1425.0
Time with inverted order.	868.7 794.5 812.0	1703.4 1649.4 1772.0	1490.0 1430.0 1407.0

As the work became more automatic the distracting effect of other ideas became less. Distinct and independent trains of thought became possible without materially interrupting the process of adding. Two subjects (Sd. and Hn.) had some of their brightest ideas come while adding, and sometimes extraneous problems would arise and be solved without causing confusion. When the full force of consciousness was needed to make combinations of numbers unusually difficult, it seldom failed to arrive at the proper time.

There can hardly be any question—at least in my own mind—as to where the attention was when the adding was going on automatically and the mind was solving some external problem, say in regard to finances. No doubt, frequent fluctuations occurred between the two; but there must have been—and the introspective record confirms this—considerable intervals in which the adding was entirely subconscious. The attitude of the mind towards the adding in such intervals may be called automatic attention, and may be said to have the same relation to sub-consciousness as attention proper has to consciousness.

But to call this automatic attention attention proper is a misnomer. It is also evident that sense organ adjustment was in this case with automatic attention, rather than with attention proper.

Experiment C.

The irregularity with which the mind works when confined to a narrow field of activity was also studied by means of nonsense syllables. Carefully selected nonsense syllables were arranged upon cardboard slips, ten syllables on each slip. Each slip was exposed for twenty seconds, during which time the subject learned as many as possible. The subject then repeated aloud as many as could be remembered, the number of correctly pronounced syllables being recorded by the experimenter. After an interval of seventy seconds another slip was exposed, and so on for a period of fifty to sixty minutes. The exposures and intervals were controlled by clockwork, and were therefore constant.

There were three subjects in this experiment, two of which (Gy. and Ly.) had had long practice in similar work, and could therefore memorize much more easily than the other.

Considerable irregularity was found in the power to learn the different slips, but there was no way of making the work necessary for learning the different slips equal, as was possible in the work of the last experiment. The experiment, therefore, was not continued for a long time. The following table gives the number of syllables correctly learned in each successive slip in a sample trial by each of the three subjects. The variations from slip to slip seemed principally due to the variation of subjective conditions, although Gy. was not consciously affected by fatigue and seemed to remember the syllables in direct proportion to the facility with which he could make associations with them. He was unusually proficient in this; and although the syllables were selected with a view to excluding associations, the results taken from the other subjects are also probably affected to some degree by this. Ly. found himself resorting to artificial means for memorizing at the ends of the periods, such as pretending surprise at some of the syllables. At such times the fatigue was very marked. This, with the fact that Gy. did not feel fatigue with his elaborate system of associations, seems allied to the introduction of variety in adding in the last experiment. With Ly. no explanation could be given by him of the variations of proficiency except an infrequent association. To him the experiment was fatiguing only when the interval of seventy seconds was considerably decreased. Gy. was not affected by this.

TABLE III.

NUMBER OF SYLLABLES CORRECTLY LEARNED.

	Ly.	Gy.	Lr.		Ly.	Gy.	Lr.
1st slip	8	9	6	18th slip	6	8	4
2d "	5	10	5	19th "	7	8	4
3d "	8	8	5	20th "	4	6	4
4th "	7	10	6	21st "	9	9	3
5th "	9	9	3	22d "	5	7	4
6th "	8	9	6	23d "	6	10	5
7th "	8	6	4	24th "	8	9	3
8th "	9	9	3	25th "	6	10	4
9th "	7	10	3	26th "	7	7	2
10th "	7	10	3	27th "	4	7	4
11th "	9	10	4	28th "	7	7	4
12th "	8	7	3	29th "	6	9	2
13th "	8	10	3	30th "	6	10	5
14th "	8	7	5	31st "	6	10	2
15th "	5	4	4	32d "	4	9	5
16th "	7	4	3	33d "	3	10	5
17th "	6	5	2	34th "	5	7	6

Experiment D.

It will be remembered that in Experiment A there was an intensity of the alternating current of electricity found which stimulated the power of attention, and that reading aloud had the same effect upon Hh. Also that reading aloud to Sh. and Hn. caused confusion. In Experiment B it will also be remembered that with all the subjects (Hh. was not in Experiment B) fatigue was most frequently shown by the presence of disturbing ideas, and that noises and entertaining ideas would cause disturbance without distinct fatigue. The object of Experiment D was to study farther the influence of a sensory stimulus upon mental activity.

The subjects were Sh., Hh. and Hn. The work used to test the constancy of the attention was the addition sheets used in Experiment B. The stimulation used was an alternating current applied by wet cloth electrodes bound firmly to opposite sides of the left wrist. The intensity of the current was adjusted by the subject to what seemed to have the most beneficial effect upon the work. In other respects the experiment was conducted as Experiment B.[1]

With Sh. five trials were taken with normal conditions and these were followed by five with the electric stimulus. These were again followed by two trials with normal conditions. With Hn. five trials were taken when the electric stimulus was used, each of which was followed by a trial under normal conditions. In the case of Hh. the same method was followed as with Hn. except that another kind of distraction in the form of a music-box playing consecutively ten popular airs was introduced, thus enlarging each group of trials to three. Sh. and Hn. afterwards used the music-box with four trials, alternating each time with a trial under normal conditions. The following tables (IV. and V.) give the time for each trial in seconds.

There are certain prominent features found here. It is seen that with Sh. (Table IV.) the time required for the trials with electricity is less than that for the trials preceding them, with the exception of the one immediately preceding, but greater than those which follow. This evidently shows the effect of practice even at this late stage, but also an accelerating effect of the stimulus at first, and a relatively retarding influence later.

This is shown much more distinctly with Hn. Here, where there was an alternation between the normal trial and the trial

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[1] After Experiment B was finished it seemed desirable to determine whether the transition from one column to another and from one sheet to another had introduced sufficient distraction to affect the results of the experiment. Accordingly, a set of the sheets was arranged with the columns running continuous in tape fashion, in the same order which they had been added in Experiment B. Six tests upon each of three subjects (Sh., Sd. and Hn.) showed no distinct effect, except such as would naturally result at the first two or three trials from any new arrangement. This tape, instead of the sheet arrangement, was used in Experiment D.

with electricity, a striking difference is at first seen in favor of the electricity, and as striking a one seen in favor of the normal later. The last normal trial is shorter by 716 sec. than the first; while the difference between the first and last of the trials with electricity is but 290 sec.

In the case of Hh., there had been no practice in adding before this experiment was begun, although the rate was much faster than in the other two cases. Here the adding was throughout faster with the electricity.

The stimulus, therefore, had a distinctly accelerating effect upon the adding at first; and in two out of the three subjects, a distinctly retarding effect later.

TABLE IV.

Hh.			Sh.		Hn.	
Normal.	With Elec.	With Music.	Normal.	With Elec.	Normal.	With Elec.
951	808	750	1787		2121	1899
707	675	683	1701	1576	1992	1852
730	673	688	1700	1537	1576	1615
680	612	684	1641	1584	1564	1721
706	655	663	1529	1605	1405	1609
				1568		
			1528			
			1522			

Table IV. shows the time for adding with music for Hh. It is seen that the effect was an acceleration over the normal, but a retardation compared with the electric stimulus. Table V. gives the effect of music on Sh. and Hn. With Hn. the effect was first to retard, while with Sh. it accelerated the adding. It should be said, however, that Sh. took the record of Hn. the day before the first trial of Sh. was taken, and that the presence of the music was so disturbing as to make the taking of the record almost impossible. Afterwards it had but a slight effect.

It is interesting to note the influence of these stimuli upon the curves which represent the time measurements for each column as in Experiment B. In Fig. 8 are given two curves from Sh., one normal and one with electric stimulus. The time for the former trial was 1641 sec. and for the latter 1576 sec. In spite of the decrease of time the stimulus is seen to have made the curve more irregular, to make greater extremes in rate, and to make them follow each other more quickly. In Fig. 9 are given two curves from Hn., one normal and one with music. The time for the normal one was 1338 sec., and for the other 1498 sec. At this trial the music was extremely distracting to the subject, causing an emotional state comparable to that of exploding wrath. Here the effect is also to increase the irregularity of the curve, with also an increase in the proportion of long periods. The curves in both these figures are strictly typical. They were corrected according to the second system of tabulation in Experiment B, and since practice had considerably shortened the time, the further correction for the effect of practice not applied in Experiment B was used here.

TABLE V.

Sh.		Hn.	
With music.	Normal.	With music.	Normal.
1499	1531	1498	1338
1380	1509	1400	1289
1401	1362	1294	1227
1390	1520	1274	1320

With Sh. the electricity decreased the number of mistakes, while the music increased them. With Hn. the electricity increased the mistakes and the music decreased them. With Hh. the mistakes were about equal for the two stimuli, but they increased forty per cent. for the normal.

The conscious effect of the electricity upon the subjects was to stimulate and strengthen the mind, and to keep away distracting ideas. With the exception of the later trials of Hn. it did not seem a distraction. In the first trials of Hn. it seemed to fill in the voids and make the time pass more agreeably. For him the fatigue after the trial seemed less when electricity was used. A few instances in which the current was unusually strong, with all the subjects, marked trials in which the time for the adding was exceptionally short and the mistakes few. The only exceptions to this were the later trials of Hn. Practice with the current appeared to make the subjects less conscious of it.

With Sh. and Hn. the music was at first extremely distracting and produced a high degree of iritability. Sh. added nearly aloud the first time, evidently to keep the mind from wandering. An effort was also made to add as rapidly as possible in the short intervals between the tunes. Sh. also tended to fall into the time of the tunes, was accelerated by quick ones and retarded by slow ones. About three-tenths of the whole round of ten tunes were too slow for this subject. They were all played through between four and five times for Sh. and Hn. Hh. was not much affected by the music, and Hn. and Sh. were not after the first time, except that in the case of each it made an agreeable interlude.

The first effect of the music was evidently similar to that of disturbing ideas and sensations in Experiment B, in which we found that these caused long, upward sweeps of the curve.

There are at least three theories that might be advanced to explain the effect of the electricity. One is that of the reinforcement of sensation, a typical illustration of which is the power of seeing an otherwise invisible object by means of a vibrating tuning-fork held at the ear. Here the adding would correspond to the visual function, and the electric current, to the tuning-fork. But this theory would not tell us why the electric current and the music had a different effect later.

Another theory might be that the stimulus acted as a reminder, thus causing a more constant effort on the part of the subject. In support of this it may be truly said that the subject sometimes was conscious of hurrying to get through quickly and avoid the pain. However, this theory would not explain the negative effect of the current, as shown by the latter part of the records of Sh. and Hn. Besides, a special effort usually brings an enforced pause.

It has already been noted that in adding without an electric stimulus there is going on a more or less constant fluctuation of the attention to other ideas. This seems to show the necessity of a frequent fluctuation due to the fatiguing of the adding function. What is required, therefore, seems to be a rest, and the way it can be obtained the most economically will assist the adding function most. In this experiment it was observed that the presence of the electric stimulus in consciousness tended to keep other ideas from intruding.

Professor Mosso states for muscular fatigue that when 30 contractions of a muscle bring about complete exhaustion, thus making a rest of two hours necessary for complete restoration, 15 contractions, or one-half the number, cause a fatigue which is recovered from in half an hour, or one-quarter of the rest needed for complete exhaustion.[1] In other words, the greater the exhaustion the slower is the rate of restoration. It is also stated by Professor Kraepelin that mental fatigue is more slowly recovered from the greater its degree. Thus it is found that school children need more frequent and longer pauses for rest as their fatigue becomes greater, in order to keep in good working condition.[2] The frequent interruption produced by the current in Experiment D would therefore bring rest when it could be used to the best advantage, before extreme fatigue had set in.

Still another important factor may be that so uniform an activity as that of attending to an alternating current does itself quickly tire, causing a quick return to the primary occupation. Herein probably lies the reason for the different effects produced by the electricity and the music. The music gave a succession of inviting ideas suggested by the notes holding the attention longer than was needed for rest.

It may now be asked how this theory accounts for the later negative influence of the electricity. It may be seen that in both cases (Sh. and Hn., Table IV.) where this negative influence was found, the practice had reduced the time needed for adding the sheets, under normal conditions, to much less than what it formerly was. With Sh. this had largely come before Experiment D, but with Hn. it came principally in this experiment. This and the increased evenness of the normal curves (Figs. 8 and 9) show that the power for constant adding had greatly increased. This evidently means that the supply of energy for the adding had come to equal more nearly the demand. Hence the intrusion of an idea when the function for adding had not become fatigued would be a hindrance rather than a help. In other words, the power of adding had become too strong to profit by so frequent rests as were imposed by the current.

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[1] 'Die Ermüdung,' p. 151.

[2] See extract from address published in *Popular Science Monthly* for October, 1896, p. 760.

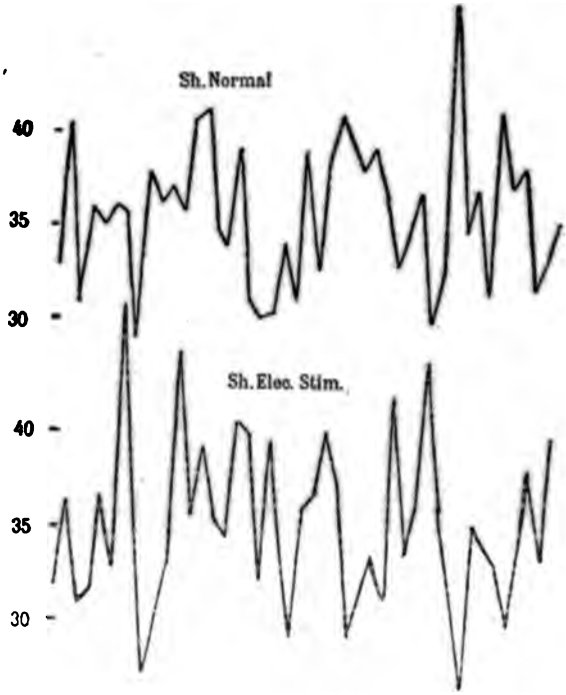

FIG. 8.

Another experiment may as well be spoken of in connection with this, as its results were of a similar kind. Sh. and Hn. tried the effect of adding the sheets under normal conditions except that a special effort was made to avoid mistakes in the sums, even at the expense of time. By comparing these trials with those preceding and succeeding, the time for adding was found to have an average increase of 131 sec. for Hn. and 123 sec. for Sh. The number of mistakes made in these trials was slightly decreased for Hn. and slightly increased for Sh. Both subjects were disturbed and confused by the attempt.

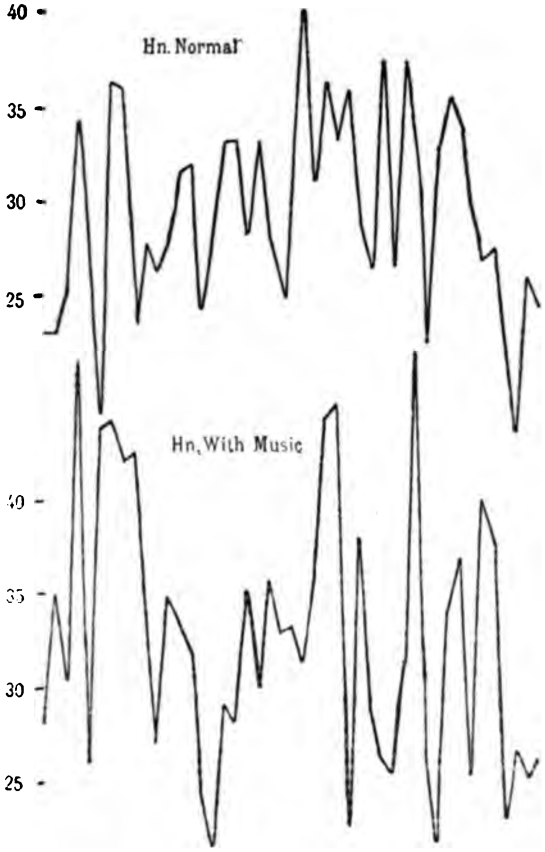

FIG. 9.

It is evident that Experiment D is unsatisfactory, since it raises an important point, but gives only a meagre amount of data: more subjects should have been used, and the work carried farther with each. There is hardly a chance for doubt, however, that what would naturally promise to be a distraction to a mental process may be of assistance. This is also borne out by the late work of Dr. Hamlin already referred to.[1] In this it was found that the power of correctly comparing distances, determined by the position of paper squares arranged upon a black screen, was increased by the subject's adding at the same time.

Experiment E.

Experiment E was planned to test the degree of independence of different mental functions, and also to show whether the exhaustion of a function at one time would be felt later, as is well known to be the case with extreme muscular fatigue.

A large black cardboard screen was fixed in a vertical position upon a table so that the light from a window would fall directly upon it. The part of the table between the window and the screen was also covered with black cardboard. In the middle of the vertical screen was an opening 6 centimetres square, on the level of the eyes of a person when seated. Behind the opening was fixed a slide for holding cards. The subject was placed 45 centimetres from the screen with the hand resting in an easy position upon an electric key which controlled a marker at a kymograph drum. Another marker registered seconds; the drum revolved once in 30 seconds. With the eyes of the subject so turned to a black square lying upon the table as to make a visual impression from the screen opening impossible, a diagram or picture was placed in the opening of the vertical screen. The experimenter gave a signal and the subject looked up, his fingers pressing the key as his eyes fell upon the picture. This was looked at as long as there was a natural inclination for the subject to do so. When through looking, the eyes went again to the black square, and the fingers allowed the key to rise, thus giving a signal to the experimenter. After an interval of 15 seconds from the rising of the key, another picture was looked at in the same way. Before each series one or two practice pictures were given, and the order of the pictures was always changed.

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[1] *Am. Journal of Psychol.*, Oct., 1896.

Each subject was practiced in the mechanical parts of the experiment to make them as automatic and unconscious as possible. As is evident, the object of the apparatus was to register the exact time—to tenths of seconds—which each picture attracted the attention of the subject. There were ten pictures in all, lettered roughly in the order of their complexity. These were selected from the stereoscopic views for giving optic illusion prepared by J. Martins-Matzdorff.[1] The letters corresponding to the original published numbers are as follows : A, No. 18; B, No. 11; C, No. 13; D, No. 5; E, No. 14; F, No. 1; G, No. 15; H, No. 3; I, No. 7; J, No. 10. The right sides only of the pictures were used except in the cases of Nos. 3 and 10, in which the left sides only were used. A to D were geometrical figures; E, concentric circles of different shadings; F, a Greek head; G, diagram of engine piston; H, silhouette of tree and gateway; I, Prometheus bound; J, colored waiter in anteroom.

TABLE VI.

Picture.	A	B	C	D	E	F	G	H	I	J
Mar. 10	2.2	4.2	4.9	9.4	10.3	12.9	26.8	18.6	23.4	10.7
" 11	1.9	3.2	4.8	2.4	8.6	11.5	18.1	9.7	20.9	7.2
" 13	1.7	2.7	3.3	1.9	8.4	11.9	3.2	9.7	23.0	12.5
" 16	1.8	1.2	2.7	4.1	77.6*	14.6	28.0	6.3	22.3	8.5
" 17	1.9	2.5	2.4	2.6	12.8	7.8	18.7	10.7	20.6	9.0
" 18	1.5	2.5	2.3	2.4	3.6	28.3	26.3	11.4	28.5	15.3
" 19	2.7	2.2	2.5	3.9	5.6	12.1	22.5	7.3	28.6	18.0
Apr. 24	4.0	2.9	2.3	3.9	4.0	10.1	119.9*	12.5	21.2	11.1
" 25	1.4	3.9	7.0	2.1	4.4	15.7	8.8	5.2	9.3	6.7
" 27	83.6*	5.4	4.0	6.3	7.9	19.9	12.9	8.3	20.2	4.6
" 28	1.3	5.1	1.2	—	1.8	3.3	4.5	2.3	8.0	6.7
" 29	.9	2.7	4.6	2.8	3.0	10.7	3.8	9.0	9.2	5.6
" 30	117.6*	3.0	129.5*	8.5	3.3	2.5	1.5	10.6	9.0	3.0
May 2	4.0	4.0	9.3	4.1	5.0	10.4	6.0	3.0	10.3	6.0

The stars denote times when the subject was required to look longer than he naturally wished to.

The artistic value of the pictures also varied considerably. The pictures were shown at the same hour on consecutive days, and the time for each picture recorded for each day. There were, however, breaks in this regularity due to Sundays and other occasional interruptions. Five subjects in all took part in this experiment. Cl. saw the series of pictures 26 times; St., 20; Dn., 19; Hn., 12; Cs., 9. Table VI. gives the first and the last parts of Cl.'s record, which, in general, illustrates the others. The numbers give the time in seconds for each picture.

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[1] Wickelman & Sons, Berlin, 1889.

Although all the pictures were of a very simple type, the more complicated ones being hardly more than outline drawings, there was yet a very marked difference of time given to the more complicated ones as compared with the simplest; and this difference is preserved throughout the experiment, even after the views had become very familiar and the time for looking at them greatly decreased. Table VII. gives the average time in seconds devoted to each view by the different subjects.[1] This phase of the experiment evidently touches upon the objective conditions for holding the attention in one direction. This experiment in itself suggests that, while the mere complication of a figure is an element in this, there are yet other important factors.

TABLE VII.

	A	B	C	D	E	F	G	H	I	J
Cl.	2.5	4.1	4.0	4.1	6.7	12.9	16.4	9.1	15.8	10.1
Dn.	7.4	6.8	8.7	8.5	10.5	10.3	9.1	8.4	11.6	8.6
Hn.	2.1	2.7	3.5	7.0	9.0	15.8	24.4	16.1	22.9	15.9
St.	19.5	17.4	21.4	28.6	30.0	36.6	33.4	37.6	41.0	43.7
Cs.	1.4	1.3	7.8	1.1	1.9	4.9	2.5	3.4	7.7	5.7
Final av.	6.6	6.5	9.1	9.9	11.6	16.1	17.1	14.9	19.8	16.8

In Table VII. it is seen that there is a great difference in the time given by the different subjects to the views, and also that a picture will attract one subject a greater relative length of time than it will another. I believe we here have a method that might be of great value in finding out not only what naturally attracts the attention, but also personal differences of mental aptitudes. While picture J was by far the most complicated of the series, picture I, of Prometheus, is seen by the final average to have had the most attractive power. If one introspects carefully while looking at a picture, a multitude of half-conscious associations are found to jostle one another along at an irregular pace, thus composing his 'stream of consciousness.' Some pass very quickly and seem to form an interlude or 'fringe' for those which linger longer and furnish more substantial perches for the attention to rest upon. Often one of these longer stops, or where the stream moves very slowly, is accompanied by a vague feeling of enjoyment when the cause cannot be understood. Perhaps more commonly one is keenly alive to the features which hold him captive, and is able to give the reason for their hold upon him. This vague attractiveness was especially prominent in the cases of the Greek head and Prometheus—those pictures of the most artistic value.

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[1] The tables showing averages do not include the time for the unnatural holding of the attention on the views to be described later.

Table VIII. gives the average time in seconds spent upon a view (for each subject) at successive trials. This shows how the time varied with succeeding days. The numbers in the column at the left of each record give the number of the day for each trial, numbered from the day of the first trial. This is to show when days came on which the experiments were omitted. With Cl., St. and Hn. there is a falling off in the time after the first trial, and a gradual increase after this. Then comes a final decrease of time.

It would be interesting to know what causes this increase. If it were not that the subjects were told to inhibit all associations—which, however, could not be strictly carried out—it would seem possible that new associations came to cluster about the views more and more. It may be that associations which were half conscious at first, or altogether unconscious, became later through the repetitions to be more conscious and thus gave an added complexity to the views. These newly made conscious elements would naturally become less and less attractive, and as the picture would have a limit to its suggestiveness, would allow the time finally to fall off.

TABLE VIII.

No. day	Cl.	No. day	Dn.	No. day	St.	No. day	Hn.	No. day	Cs.
1	12.3	1	7.4	1	24.0	1	9.9	1	5.4
2	8.8	2	9.1	2	22.0	2	6.9	2	6.3
4	10.7	4	8.6	3	17.5	3	14.3	3	4.3
7	9.9	5	8.0	4	28.4	5	7.6	4	3.5
8	8.5	8	7.7	7	32.6	6	13.8	11	6.3
9	9.6	9	9.8	8	46.6	7	20.3	13	2.3
10	10.5	11	8.3	9	38.0	8	17.8	14	1.1
14	17.2	12	9.2	10	55.5	9	9.2	17	.9
15	19.7	13	8.3	11	49.7	10	15.0	17	.4
16	13.6	14	9.6[*]	13	36.0	14	8.8		
19	8.7	15	7.5[†]	14	21.5	15	14.7		
21	5.7	16	8.5[†]	15	52.9	15	11.4		
22	6.3	25	8.6	16	43.3				
23	13.4[*]	26	9.2	17	28.3				
24	3.4[†]	28	8.2	18	11.8				
25	4.9	29	10.5	20	18.4				
26	4.8	33	9.1	21	19.2				
45	8.0	34	13.4	23	17.5				
46	6.2	35	15.2	24	22.1				
47	9.9			24	13.5				
49	4.1								
50	5.2								
51	5.7								
52	6.1								
54	6.6								
55	5.6								

Although there is a comparative distinctness in the difference of time given at each trial to the different pictures, as shown in the sample of Table VI., and still more in the averages in Table VII., this distinctness is by no means constant. Thus, while (Table VI.) *A* is generally less attractive than *B*, sometimes it is more so. This seems to show that the subjective valuations of the different views are independent and that a general increase or decrease of time is not dependent wholly, at least, upon some general cause influencing the whole series, like an unusual buoyancy or depression of spirits. If the cause suggested for the general increase of time given to a picture be true, it is natural to suppose that the increased or decreased valuation would not go on at exactly the same time in the different pictures.

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[*] The intervals between the pictures in this trial were filled with adding.

[†] The intervals between the pictures in this trial were filled with looking at magazine pictures.

Doubtless, however, there are many factors that have been overlooked as well as uncontrolled; therefore, to give a better test for the separation in the functioning of different mental activities for different views, a modification of the above experiment was resorted to. If we regard the capacity for enjoying one picture as exercising a mental function comparatively independent of that used for another picture, then if the time is lengthened beyond the natural limit of enjoyment, a fatigue would naturally result which would shorten the time for enjoying that particular picture at the next trial; providing that nutritive processes had not placed that function in its first anabolic condition.[1] Accordingly, at different trials views were selected to which the subject was required to give close attention for a period of time controlled by the experimenter. All associations were inhibited so far as possible, although Dn., Cl. and Cs. were unable to do this satisfactorily. Table IX. gives the record of these trials in seconds in their order. The figures in heavy type give the number of seconds the subject was required to keep the attention confined to the view, while the numbers above these give the time the view had naturally attracted the attention on the two preceding trials, and those below the time it attracted the attention at the succeeding trials. As a rule the simpler views were taken for this. The letters at the top of each column tell the particular view.

The effect of this is seen to vary. Often the effect of the first trials was to increase the attractiveness of the view, bringing out many new minor points even after the views had been looked at many times. With two subjects (St. and Hn.) distinct aversions or passive dislikes often appeared for the view in the trials succeeding those of the long periods, even when the subject had forgotten what view had been looked at in this

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[1] It is evidently incongruous to speak of mental functions as being dependent upon anabolic conditions when psychoses have not been proved to result from neuroses. It has been shown, however, that the functionings of the two are controlled by similar if not identical laws; so that while the terms applying to one may not apply fully to the other, yet they are at least convenient figures of speech.

TABLE IX.

	E	B	C	F	A	A	C	C	J	
Cl.	8.6 8.4 **77.8** 12.8 3.6	2.5 2.2 **68.0** 10.2 32.4	2.2 4.9 **125.0** 1.9 7.3	11.8 12.6 **119.9** 8.8 12.9	4.0 1.4 **88.6** 1.3 .9	1.3 .9 **117.6*** 4. 3.	1.2 4.6 **120.5*** 9.3	4.6 9.3 **115.0** 3.0	3.0 6.0 **240.0** 20.0	
	E	C	A	C	E	F	D	G	A	B
Dn.	15.2 10.1 **46.1*** 10.8 7.9	8.0 10.1 **122.0** 8.0 9.5	7.4 8.0 **78.2*** 7.3 9.4	8.7 6.0 **77.7*** 5.5 11.6	7.4 10.0 **88.2*** 10.4 12.0	10.0 13.8 **72.6*** 7.0 10.3	8.0 8.4 **55.0** 9.6 12.0	11.9 10.1 **47.8** 10.8 7.3	7.3 9.4 **85.1** 7.3 11.5	10.1 7.6 **82.4** 12.6 8.7
	A	D	D	C	D	A	H	H	D	I
St.	17.7 11.0 **115.4†** 14.1 13.0	15.2 21.0 **42.8** 32.0 41.0	41.0 59.1 **180.0†** 21 31	46.0 46.0 **180.0** 28.0 52.0	14.0 75.0 **240.0†** 8.0 10.0	17.0 8.0 **60.0†** 6.0 7.0	28.0 11.0 **240.0** 14.0	11.0 14.0 **240.0** 25.0 25.0	19.0 24.0 **120.0** 12.0	28.0 16.0 **180.0** 14.0
	C	A	C	A	I	D				
Hn.	1.6 3.4 **46.1** 2.5 7.9	1.5 1.9 **191.6†** 1.6	2.5 7.9 **111.2†** 5.0 1.9	1.6 **52.0†** 2.0 2.3	11.0 46.8 **240.0** 1.0 5.5	11.0 3.0 **278.0** 2.5				
	C	C	A	I						
Cs.	2.9 1.6 **44.0** .6	.6 33.8 **42.0** 1.9 .8	1.0 3.0 **189.0*** 1.2 .5	5.3 1.2 **51.9** .6						

Those marked with a star were not followed by a trial on the succeeding day. A dagger marks the cases which were followed by distinct distaste.

way. Cl., Dn. and Cs. felt no distinct differences in the succeeding trials from this enforced attention, though all but Dn. and Cs. felt fatigue from the effort at the time. These continued to see so many new things in the views and had such imperative associations that the attention could not be kept fixed. It is interesting to note that St. and Hn. were the only ones successful in inhibiting associations, and also were the only ones who felt the distinct distaste in the trials following the long periods. Evidently the rest which came from the variety in the associations had to be eliminated to make the effect of the fatigue lasting.

There were very distinct emotional states accompanying this experiment. Beside those vague feelings of recognition already described, there were those of active enjoyment which usually accompany the looking at pictures, and which gradually grew less as the series was repeated. There were also the feelings accompanying fatigued functions peculiar to the modification of the experiment just described. We have seen in Experiments A and B that a degree of fatigue is easily reached which makes constant work impossible. In the present experiment we seem to have the natural emotional accompaniment of that condition. The milder stages of it are shown by a lack of response to the object on the part of the subject. There is the feeling of, *there is nothing in that for me*. It is like a dry mouthful of something that cannot be masticated. This stage gradually merges into that in which the object is positively distasteful or repulsive. This is often tinged with a kind of sickening dread, comparable —if I may carry the illustration still further—to the feelings accompanying the eruptions of a rebellious stomach. One subject said the sight of the object caused him to shudder and to say impulsively, "Take that out."

A similar experiment was tried on St. with the music-box used in Experiment D. Each tune was repeated at the first trials as long as the subject wished it. At later trials a tune would be repeated several times after the subject wished it stopped. At first this seemed to increase the attractiveness of the tune, as was the case with the views. After several trials of this kind, however, the repeated tune caused the same revulsion and shudder that was noted with the views. The experimenter had a distaste for the repeated tunes even before the subject.

In this last phase of the experiment there appeared more distinctly than before those vague feelings of enjoyment noticed especially with Prometheus and the Greek head. Such feelings seem to merge gradually into the more definite associations which accompany recognition, and these associations in turn branch out in new directions suggested by the object recognized, whether it be a tune or picture. No definite line can be drawn to divide the different stages of this process. It may be of significance that these variously branched associations have the same effect of prolonging the natural time for attending to an object that the variety in the ways of adding introduced into Experiment B had in making the rate of adding more constant; and that the complexity of an object has the same effect. The evidence from these observations seems to indicate: first, that an object of attention holds its ascendency in the mind longer (*a*) because of its complication or variety of elements; or (*b*) because of the associations formed with it (this last would include its æsthetic value); and second, that the process and irradiations of thought are to a considerable degree controlled by fatigue; since the inhibition of them seems to result in the excessive exhaustion of some mental function. If this is true, it is evident that the more volition there is employed in attention the more the equilibrium of the mental activity is disturbed.

A source of inaccuracy in this experiment was the inability of the subject to give an exact limit to the time during which he wished to look at the views. It was easy to tell when that time was not nearly expired, and when fatigue had definitely set in, but since one shaded into the other gradually, no exact line could be drawn between them. When the mind was in a torpid state this transition was much longer than at other times; and it was longer with some subjects than with others. In general, it may be said that the longer the time a view was naturally looked at, the longer was this transition period. The relative error would, therefore, be fairly constant. With the longest times this usually was not over five seconds, and for the shorter not over one-half a second.

Experiment F.

Experiment F was designed to make a still further test of the separation of mental functions. On one day the subject added as rapidly as possible the addition sheets of Experiment B, twice in succession. On the next day nonsense syllables were learned for a period of time equal to that required for adding the sheets the first time on the first day, and this was immediately followed by adding the sheets once. On the third day the sheets were added twice again, and so on. The same nonsense syllables were used as described in Experiment C. In Experiment F, however, each slip was studied until it could be repeated correctly before another slip was learned. Usually five or six slips were learned in the time allowed. Complete records were taken from two subjects only, Sd and Hn. The former added the sheets twice in succession ten times, and added the sheets preceded by the syllables ten times. The latter did this but five times. The following table gives the results. The work was done at the same hour each day in the case of both subjects.

TABLE X.

	No. of Trial Group.	Time for adding Sheets twice in succession. First Time.	Time for adding Sheets twice in succession. Second Time.	Time when adding was preceded by syllables.
Sd.	1	1038	1094	921
	2	921	929	892
	3	922	998	875
	4	928	984	819
	5	912	922	871
	6	925	975	903
	7	889	935	815
	8	915	960	869
	9	872	918	870
	10	867	926	859
Hn.	1	1203	1282	1088
	2	1172	1296	1104
	3	1184	1173	1028
	4	1077	1109	1051
	5	1055	1095	1014
Totals.		14880	15596	13979

It is seen by this table that the number of seconds for adding the sheets the second time, when the adding was immediately repeated, was considerably greater than for adding the first time; and that the time for adding the sheets after the syllables had been learned was less than that for adding the first time by an even greater difference. There is no exception to this rule. Another subject (Lr.) was started upon the same experiment and showed the same features in his result, but, as the work was not long continued, the results are not given. The following record of mistakes made in the adding is seen to be in perfect accord with the time measurements.

TABLE XI.

		Sd.	Hn.
Whole number mistakes in adding sheets twice in succession	1st time	42	55
	2d time	53	60
Whole number mistakes in adding sheets when preceded by syllables		30	38

So far as the subjects were conscious of the effect of learning the syllables upon the adding, it kept the mind freer from extraneous ideas, and gave an agreeable change in turning from the syllables to the adding. These tables show not only that the adding function was not fatigued by the learning of the nonsense syllables, but that its power was materially increased. The learning of nonsense syllables may be supposed to exercise but a limited range of mental activity, and thus keep the attention from innervating the store of energy used for a different kind of activity, more effectually than would involuntary musing. That this increase of power was not due to a general 'warming up,' or to increased excitement, is evident from the fact that when the sheets were added twice in succession the second adding was slower; and also that at several trials when the pulse was taken before and after the work it was found always to be slower at the end. The emotional effect of changing from the syllables to the adding was also typical of the activity of a high anabolic condition for the latter. This experiment, therefore, very strongly confirms the theory set forth above. It also contains a suggestion for a method of controlling subjective conditions for experimentation. If the confining of the attention to one mental function causes another function to be supported by an amount of energy which, when discharging, excludes all irrelevant ideas that would otherwise be a distraction, then such a device for conditioning a function to be measured would doubtless be of considerable value.

It is not only towards objects of attention over which we try to fatigue ourselves that we find changes of mental attitude taking place. We seldom have difficulty in sitting down to the dinner table to decide what dishes are best suited to our appetites; and we also notice that what would be enjoyed one day might be nauseating on another. Our intellectual enjoyment of the different objects of our attention is in many ways similar to this. It is not difficult at any time to tell whether a certain mental activity (such as the rehearsal of a poem, or the recollection of a past experience) would be enjoyable or otherwise. And it often occurs that we have a feeling of satisfaction after such an activity comparable to that feeling of contentment which follows a good dinner.

Experiment G.

In Experiment G the data consist chiefly of emotional records and are made up of experiences like those first cited in the introduction, which give us the most diverse aspect of our topic. Is is evident that this was not a laboratory experiment. Several friends, interested in psychology, the most of whom were members of Clark University, were asked to select each an object in his mental environment towards which he was conscious of an occasional change of feeling. They were asked to describe each change of feeling, to give the date, and, when possible, to give the cause of it. Eight persons responded by handing to me, some months later, detailed accounts of their experiences. There were fourteen separate records in all, some selecting more than one subject. One record covered a period of eight months; six, four months; two, two months; two, one month; while the others were general accounts or simple instances of one change of feeling. Of these, six represented topics of research; two were changes in taste for food; two, family affection; one, physical exercise; one, attitude towards table board; one, the progress of a love affair; while a lady selected a piece of embroidery upon which she worked.[1] These records will require closer examination.

Table XII. gives the duration of each phase of feeling in days for the records of those who selected for their subject their topics of research. The figures in the + columns denote the days in which a liking was felt for the subject; those in the ± columns days when there was neither like nor dislike; and those in the — columns when the work was distasteful. The true sequence of these periods for each subject is shown by the vertical order of the figures. This table does not give the minor phases of these fluctuations so completely as would be desired. Positive, negative and indifferent periods are all subject to variation of intensity in the feeling, thus often making it difficult to draw fast lines between them. When, however, any unusual strength of feeling was experienced it is shown in the table by an 's' placed under the number.

From this table it is seen that frequent changes of feeling were experienced. The causes of these as given by the subjects were of two kinds. First, the state of health; and second, the encouragement or discouragement from the value of the material worked upon, or the tone of a consultation. Sickness or loss of sleep or appetite usually brought a passive or negative feeling, though this was not always the case. Cases in which the lowering of the physical tone had this effect may evidently be classed with those in which fatigue appeared in a diminished or changed activity (Experiments A, B, C, D) or distaste (Experiment E), since we may suppose that the nutritive functions were interfered with, the supply of energy thus being made less than the demand. The positive feeling which resulted from rich material or encouragement resolves itself into pleasant associations with the work, and can therefore be classed with those cases in which the numerous associations and complexity of the object of attention caused it to be more attractive (Experiment E); while discouraging conditions would naturally make the subject distasteful from its association with them. When the fluctuation was not attributed to ill health or discouragement, it is my hypothesis that it was due to the exhaustion of the function exercised.

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[1] I cannot express too heartily my indebtedness for the expression of confidence involved in submitting some of these data. The opening of the sanctuary of the human heart for purposes of exact knowledge shows not only a confidence in the future of psychology, but also a confidence in the possibility of personal integrity.

TABLE XII.

Subject.	A			B						C			D			E		
Attitude.	+	±	−	+	±	−	+	±	−	+	±	−	+	±	−	+	±	−
			1			4	(continued)			7					¼	½		
	1			1					1			2	¼					2½
			7	s	1			1		2½				1½			19	
	s																	
						3			3			2½			1	3		1
	6		5	4		s		3		22		s		1				
	s			s		1			2			2			2	3		
	23		1		2			2	s	2			8					2
		1				1	1								1		1	
			6	2				3					1		s	5		
				s	2		2									s		
	2		1	6			s	1										
						3			1									
	8			9		s		3										
	s					1	1											
					3	s		1										
				5					1									
						5		1										
					2		1											

There may be fluctuations other than those of intensity while the attitude still remains positive, as the following quotation from one record illustrates:

"There are undoubtedly fluctuations in my interest in my present work. As soon as my plans were formed in the fall I started off with elation of spirits. In perhaps a month I found myself dissatisfied with what I was doing and my interest at zero. Then came a new impulse just before the holidays, which I am glad to say is still with me.

"Within this larger curve there are smaller ones. My interest changes from one phase of my work to the other. Now I give my attention to psychology and I read with avidity

James, Ladd or Wundt. Then I find myself losing interest in this work completely, and will turn to pathology and read Clouston or Maudsley just as eagerly. Then, again, I will surfeit myself with such reading and turn to Whitaker or Foster and read neurology for several days."

Prominent among the terms describing the mental attitude are the following: 'strong revulsion,' 'revulsion,' 'disgust,' 'indisposed to work,' 'neutral,' 'indifferent,' 'decidedly more interest,' 'deep interest,' 'very high interest,' 'strong desire to be at work.'

Following are the cases of change in taste for food:

"When I was 10 or 11 I became very fond of tomatoes, stewed, and sliced with sugar, salt and pepper on. This fondness continued for 10 or a dozen years, and only diminished because it could not then be gratified. I am still fond of them, but not extravagantly any more."

"In 1892–3 I had a spell of liking candy (gum drops, in particular). It disappeared until a month ago, but is not so strong as three years ago."

I believe it is a well-known fact, although I have no specific data for it, that an over-indulgence in an article of food often causes a distaste for it afterwards. I am told that one of the popular cures for inebriety is a forced diet of food and drink always flavored with intoxicants.

The cases of family affection were both records from the same person in respect to two members of his family. The best way to give these is to copy the record. In Table XIII. x stands for one person, and y for the other. A high degree of positive feeling is denoted by additional plus signs and negative by negative signs; indifference by plus-or-minus sign. This is but a fraction of the record, but it illustrates the whole. Whenever a day came which especially reminded this person of pleasant family associations, the positive feeling was greatly increased. A letter announcing the sickness of one caused the strongest positive feeling for both members.

The record of physical exercise shows considerable variation. When not feeling well, or when other objects of interest were unusually absorbing, it was neglected, sometimes for three or four days. Sometimes a high state of vigor caused it to be enjoyed for an unusual length of time. Although a parallel record was kept for other things, there seemed to be no constant relation between a positive or negative feeling for one and a similar feeling for another; but more extensive data would, of course, be necessary to settle this point.

TABLE XIII.

Date		*y*	Date	*x*	*y*
Dec. 29	+	—	Jan. 9	+	+++
" 30	+	+	" 10	—	++
" 31	++	+	" 11	+	+
Jan. 2	++	+	" 12	++	++
" 3	—	±	" 13	±	±
" 4	—	±	" 14	—	—
" 5	—	±	" 15	±	—
" 6	—	——	" 16	—	±
" 7	—	+	" 17	—	+
" 8	—	++	" 18	±	++

The record of attitude towards the boarding place was controlled much by the dishes served each day, but not wholly.

One describes a periodic hunger for items of news lasting for several weeks at a time, and then says:

"Resembling the above is my periodic appetite for current literature. This finds its gratification in reading magazines and short stories. It would be represented by a much larger curve than that of my news hunger. Intervals of months will elapse during which I will be indifferent to magazine stories or poetry. Then I will suddenly be possessed of the desire to get in touch with literature again."

The piece of embroidery was selected with special reference to this experiment. The design was that of a vine with leaves, and was worked in different shades of green to diminish the possible effect of a variety of colors. One long period of decreased interest lasting a month, during which the interest at times was entirely lacking, was caused by the pressure of other work. During this time but little work on the embroidery was done. Other causes of decline of interest were a feeling o general indisposition, and the working of the stems of th leaves which was not so pleasing as the rest.

More extreme feeling would naturally appear where emotion plays a greater rôle than in the cases described last. I will quote from the case which illustrates the source of so much mental tragedy and delight—the case of love. I will change the names to avoid identification.

"*October 15.*—I believe the very essence of *A*'s character is so imprinted in me that I judge other girls by her. She is very different from Miss *B*, and I think the difference is favorable to *A*.

"*October 16, 7:15 A. M.*—I half fear the housemaid across the hall will enchant me if I cannot anchor my thoughts upon somebody that is better—though I don't know but what she is all right. I have half a mind to appeal to *A*'s sympathies directly.

"*7:45 A. M.*—Even now, in writing a business letter, I feel less dependent upon *A*, and a stronger impulse towards an absolute ideal.

"*6:30 P. M.*—The maid across the hall is not so pretty as I thought, and I am more strongly attracted to *A*.

"*October 17, 6:50 A. M.*—I had a sort of Alastor's dream last night in which I saw two girls, one of which—dressed in black and a brunette—was very beautiful, and more passive and serene than *A*. I think I can worship an abstract ideal without the aid of flesh and blood at present. Yet *A* has a beauty that I cannot expect to see equalled.

"*11:00 A. M.*—I think I could live perfectly happy if I were never again to see *A*.

"*5:00 P. M.*—*A* seems like an enthroned angel whose skirt I am not worthy to touch. I can never find anyone half so good.

"*6:00 P. M.*—When I went down stairs to-night the maid was not there. I guess she is gone. I am conscious of an impulse towards *A* in consequence, though I have a kind of tasteless exhausted feeling when thinking of her.

"*October 18, 7:45 A. M.*—How I wish I could get into perfect sympathy with *A*. I am sure I should find a tender, gracious heart. Her stiffness comes from only a feeling of awkwardness from our separation.

"*October 18, 12 M.*—I have been thinking all the morning that I am the unworthy one rather than *A*. I did not show enough consideration for her feelings when I called last. I had too much the attitude of a critical instructor, and have not credited her with half enough of the things I wished to see in her. I see nothing whatever to blame in her. Her negative way of looking at things that interest me is simply a frank confession of lack.

"*October 19, 8:30 A. M.*—*A* has not appeared so prominently this morning. She still has that sacred, semi-divine aspect, and I see nothing in her but to admire.

"*October 21, 8:30 A. M.*—*A* holds the same place now. Last night I enjoyed quite a little feast in thinking about her.

"*October 22, 8:30 A. M.*—*A* has much the same position, though perhaps I have a bit more familiar feeling for her.

"*4:30 P. M.*—This familiar feeling—if it may so be termed —has been increasing somewhat. She no longer has that sublime enthroned aspect, but is seen as if at a nearer view. Her actual appearance, the slight blemishes of the face, and the little ways of speaking, and little movements all come out distinctly. I have a wholesome taste for her society, though I do not feel like placing her so highly as before.

"*October 23, 7:00 A. M.*—Neither like nor dislike for *A* this morning.

"*October 24, 7:00 A. M.*—More liking for *A*. Lately I have not thought of her so often as a few days ago. Now, only three or four times a day perhaps; then almost all the time.

"*October 25.*—If I do not hear from *A* pretty soon in a favorable tone I think I shall put the case of our relationship before her as candidly as I can—see if I may not expect more than a mere friendship.

"*October 26, 8 A. M.*—Yesterday afternoon I was conscious of a slight reaction. It amounted to questioning once or twice whether *A* is really well suited for me."

I am informed by the author of this record that during the time represented by the above extract no letters or calls were exchanged between him and the object of his affection. The variations of feelings could, therefore, not have been influenced by any encouragement or discouragement given him at the time. The record continues for several months, but the above extract illustrates the whole.

There are certain well defined characteristics to this experience. In the first place it is evident that the subject had a ripe desire for the society and reciprocation of feeling of some one like his friend. This desire may be taken to represent a function which, not finding an opportunity for its complete activity, exhausts the power of mental imagining in the effort. Following in consequence of this fatigue comes:

First.—The shifting of images, as when the 'semi-divine' aspect changes to a more familiar one which develops the various personal features and characteristics. In other parts of the record is given a variety of these aspects. The usual image of the body sitting or standing in a certain position is sometimes maintained without change for several days; then the position, facial expression, or costume will be varied, thus placing her in a variety of aspects. This, no doubt, is caused in the same way that the variety in adding was brought about in Experiment B. During this time the feeling was positive; but its intensity is seen to decrease when other interests are strong, as when the business letter was written and when other persons or ideals made a strong impression. This is certainly similar to the intrusions of external interests in Experiment B.

Second.—There were the periods of inactivity when neither a positive nor a negative feeling was present, and,

Third.—Times of distinct aversion. These will be recognized as states similar to those induced in Experiment E, and no doubt represent degrees of distinct fatigue.

The relation between feeling and reason is presented in a novel way by this record. When a strong positive feeling was present, reasons for negative feeling could produce little or no effect; and also when a negative feeling was uppermost, reasons for the positive were without effect. But when a low intensity of either was felt reason had a greater influence. Often, however, feeling leads and reason tags along after, as is illustrated by the entries of October 18. While the excitation of a new interest is several times spoken of as lessening the positive feeling for '*A*,' a disappointment or the failing of some interest had the opposite effect, as is illustrated by the rise of feeling for '*A*' when the housemaid was found to be less attractive than was supposed. Different interests thus seem, temporarily at least, to work at cross purposes, each trying as if to draft off all the mental energy in its own direction. If reason is a comparison of ideas, and ideas are articulated feeling, and feeling in turn is determined by the distribution of brain energy, these relations of feeling and reason may no doubt have an explanation.

The intimate relation between thought and feeling here suggested is by no means a new idea. "Not only do feelings constitute the inferior tracts of consciousness," says Herbert Spencer,[1] "but feelings are in all cases the materials out of which, in the superior tracts of consciousness, intellect is evolved by structural combination. Everywhere feeling is the substance of which, where it is present, intellect is the form. And where intellect is not present, or but little present, mind consists of feelings that are uniform or but little formed." Mercier[2] believes that "The feelings of belief, doubt, perplexity, conviction and several others are, on their reverse side, cognitions, and may correctly be regarded either as cognitions or as feelings according as we view them on the reverse or the obverse."

In the cases of all these records we get a more or less frequent change of feeling. We should not be warranted in saying that a negative feeling always follows in consequence of exhaustion due to positive activities, or that the intensity of a negative feeling is in proportion to the intensity of a preceding positive one, although there is some tendency for this to be true. There is more reason for believing that in general a positive feeling indicates that the function exercised is supported by a good amount of nervous energy, and a negative feeling,

[1] 'Principles of Psychology,' Vol. I., Part II., Chap. II., § 76.

[2] 'The Nervous System and the Mind,' p. 227.

the opposite condition. The influence of associations is also very marked, as if pleasing ones increased this energy and unpleasant ones detracted from it.

PART III. CONCLUSIONS.

The data here collected are naturally grouped about two points: first, the laws controlling the fatigue and recuperation of a mental function; and, second, the independence with which different mental functions operate. Experiments A, B and C have to do principally with the first; D, E and F, with the second. In G we find these two principles combined.

1. Fatigue and recuperation evidently proceed by the same laws for both mental and muscular functions, this being more apparent the more restricted the range of the mental function studied. Fatigue causes a decrease or cessation of the primary activity, thus allowing a secondary or comparatively separate one to come in and thus cause a fluctuation of the attention.[1] The more interesting the secondary idea or the stronger the disturbing sensation the less is the degree of fatigue necessary for this displacement.

2. The more complex the object of attention, the greater its æsthetic value and richness of associations, or the more it chances to meet the particular taste of the individual, the longer will it hold the attention.

3. Positive and negative feeling may result from metabolic conditions and strongly influence reason. Reason seems to be of the very stuff of which feeling is made and to represent that part of feeling which is under voluntary control.

4. From Experiment B it would seem that a mental function may be developed through the invention of economic devices and the increased power of holding mental images.

An experiment lately conducted by Mr. D. D. Hugh incidentally illustrates this last point. An oblong opening in a screen is placed horizontally before a horizontally placed cylinder, which is wound spirally by tape. When the cylinder revolves, the subject, placed before the screen, sees a series of oblique strips of tape moving in one direction. The cylinder was then also wound by tape in the opposite direction so that in revolving, a series of oblique strips seemed to pass the first series, going in an opposite direction. The subject was required to follow one series with the eyes and to disregard the other. At first there was a frequent alternation from one series to the other in spite of the subject's efforts to keep the attention upon one. By continued effort and practice, however, the periods between the alternations gradually increased until, after a time, either series of strips could be disregarded while the attention was confined to the other.

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[1] Dr. Theodate L. Smith, who has lately been working on *the motor element in memory*, informs me that fatigue and poor physical condition have been found by her to decrease greatly the power of continued attention, giving a large mean variation in the results of her experiments.

Another illustration is furnished by recent work of Dr. E. H. Lindley.[1]

The subject, either with eyes closed or surrounded by a uniform white canopy, was required to hold in mind a simple visual image like that of a red cross on a blue background. At stated intervals this was changed for the reverse image, a blue cross on a red background. The intervals between the changes were ten seconds. At first either image could be held distinctly and constantly for only a part of this time, but after considerable practice either could be held for more than eight minutes.

In arranging the data of Experiment G it was striking what apparently slight items seemed to change the entire directions of one's thoughts and the nature of one's mood. A few sympathetic words, a chance idea, the incidental discovery of a new point in the policy of a person or institution, or even an imagined personal attribute was enough to determine one's mood for days or weeks. The question suggests itself whether such influences and even much slighter ones may not be brought under judicious control for the advantage of the mental and moral victims of our physical and social conditions.

PART IV. THEORETICAL.

It may be that a somewhat strained effort appears in the endeavor to reduce all mental functions to the same formulas here arrived at; yet it seems safe to postulate on the basis of the foregoing data, and what is known of cerebral localization, that the differences in the objects of our attention correspond to differences in the nature and location of the required brain activity, and that emotional colorings result largely from this and the intensity of the activity present.

The idea of cerebral localization for the different senses and for different ideational processes is by no means new. The centre for sight has been located in the angular gyrus about the posterior end of the parallel sulcus and in the occipital lobe. Ferrier has localized the centre for hearing in the first temporo-sphenoidal convolution. Bilateral lesions of the first and second temporo-sphenoidal convolutions in man cause complete deafness. Anatomical considerations and direct experimentation place the olfactory centre in the anterior extremity of the temporo-sphenoidal lobe. The sense of taste is thought to have its centre closely related to that of smell, and is believed by Ferrier to be near the lower extremities of the temporo-sphenoidal lobes. Certainly the inability to outline exactly the areas for the various sensory and motor centres does not show that these centres are not definitely organized, but that they are diffused, as might be expected, rather than confined exclusively to any one locality. Professor Flechsig[1] has found a number of association centres not immediately connected with the sensory and motor areas. Two-thirds of the human cerebrum, he says, is taken up by these. The region they represent is made up by the frontal lobe, a great part of the temporal and posterior lobes, and extends deeper into the brain to include the island of Reil. These parts seem to have to do only with internal adjustments, and may be called the 'association or coagitation centres.' "It is particularly the disease of the association centres," he continues,[2] "that causes insanity; they are the special objects of psychiatry. We find them changed in those mental diseases the nature of which is made clear to us because the microscope can distinctly discover the cause for change, cell for cell and fibre for fibre; and so we can show directly what result it has for the mental life when they are either too much or too many of them disorganized, or both."[3] According to Ferrier, "There are centres for special forms of sensation and ideation, and centres for special motor activities and acquisitions, in response to, and in association with, the activity of sensory centres; and these in their respective cohesions, actions,

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[1] Through the courtesy of the above named gentlemen, I am allowed to refer to their work while it is still unpublished.

[1] 'Gehirn und Seele,' by Dr. Paul Flechsig.

[2] *Loc. cit.*, p. 24.

[3] In a note the author admits this to be theoretical.

and interactions, form the substrata of mental operations in all their aspects and in all their range."[1] I will also quote from Dr. Hyslop, who says:

> "If we accept the doctrines of the more recent English school—that individual sensations or ideas exist only as members of a connected, conscious series, and that consciousness, therefore, can never be conceived as a mere sum or mere product; and if we believe with Hume that consciousness is a mere succession of ideas without inner bond or connection, or that it is the series of our actual sensations (John Stuart Mill)—it may be thought possible that there are individual nervous elements which possess isolated and distinct forms of consciousness."[2]

In the light of this support from both psychologists and neurologists it cannot be called radically theoretical to regard experiments E and F as supporting the idea of cerebral localization of the higher functions. This is demonstrated in a practical way whenever we vary our pursuits to avoid their monotony, seek recreation, or take up a new fad. In ordinary life our attention is constantly being diverted by disturbances and the necessity of attending to routine duties. Sometimes these intermissions are sufficient altogether to prevent pauses resulting from fatigue; and when they do occur we simply say we are tired of writing, or reading, or staying at home, and very wisely take diversion as a matter of course. Such diversion, it would seem, serves to allow the exhausted function to recuperate, so that one's normal pursuits may be safely taken up at a later time.

The neurological basis for the fact that a complicated object holds the attention longer than a simple one may be that there are distinct nerve elements which are exercised by each of the qualities an object possesses, and that while some of these elements give us the consciousness of one part of the object, those corresponding to another part are resting, and so preparing to take up their work when the first elements become fatigued. In this way the more complicated the object, the less would there be required of the individuals in this division of labor.[1] This hypothesis requires that the mind does not hold all of a complicated object in consciousness at once. A little reflection only is required to convince one that all objects of attention, however simple, are really made up of different qualities that are easily distinguished. By holding the attention upon an object having but a few of these, he is readily impressed with the extreme shortness of the time that each can appear continuously. Let the reader concentrate his attention upon an imaginary just visible grain of dust suspended in a permanent position two feet before his eyes, and with simply the qualities of color, and position relative to himself. At first, by the effort to visualize, it is perfectly distinct, but it is only for an instant; then it either disappears or takes on qualities of motion, colors, or some other imagined elements. "There is no such thing as voluntary attention sustained for more than a few seconds at a time," says Professor James.[2] In working upon reaction times Professor Cattell tried delaying the signal preceding the stimulus for the reaction, thus keeping the subject in a state of strained attention. It was found in this way that the time of the reaction lengthened after one second.[3] What the time would have been if the object of attention had been reduced to its barest possible dimensions can only be inferred. But any one who tries the experiment with the grain of dust, just described, will be convinced that it would be very short. It would be of great value to know whether all of these qualities in perceiving an object pass through the mind singly or are fused. If they pass singly —and I am inclined to this opinion—they pass so quickly as to give the impression of being fused. I believe it is an error, however, to say we can divide the attention between two objects at the same time, although this is a popular theory among psychologists. An experiment supposed to prove this is described by Wundt.[4] By means of a falling slide, several letters of the alphabet are presented to the area of clearest vision for about 0.08 of a sec. After this short exposure the subject is able to name four to six of the letters. This, accordingly, is supposed to show that the attention can be divided between four to six separate ideas at the same instant.

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[1] 'The Functions of the Brain,' p. 147.

[2] 'Mental Physiology,' p. 113.

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[1] In the work of Dr. Lindley, already referred to, it was found that, while a very simple object could be visualized for but a short time, a complex object could be retained indefinitely.

[2] *Psychology*, Vol. I., p. 420.

[3] *Mind*, Vol. XI., p. 240.

[4] *Physiologische Psychologie*, Vol. II., p. 291.

I have tried a similar experiment by showing to the subject small colored discs arranged upon a screen behind a revolving shutter, which allowed an exposure similar in length to that of Wundt's experiments, and required the subject to give the number of discs seen. I obtained a result similar to his, but observed a period of hesitation between the exposures of the disc and the verdict of the subject. This led to the question as to whether the perception of these different objects was really simultaneous or consecutive. That is, was not the mental after-image retained long enough and with sufficient freshness to allow the attention to pass quickly from one to the other and so perform a process of rudimentary counting? This after-image is a sort of mental photograph of the objects, which fades after a moment, and not like the more elaborate mental reconstruction which Wundt distinguishes as a later and longer process. If this is the case, the number of separate objects remembered simply gives the number of consecutive objects towards which the attention can be directed during the short time before this impression fades.

For the purpose of getting another method of deciding whether the attention can be divided I arranged upon a horizontally revolving kymograph drum a belt of paper which carried on it two series of upright lines arranged in the following way: (*d*, *e*, Fig. 10.)

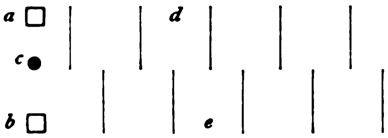

FIG. 10.

a and *b* represent openings of the same size in a screen placed vertically before the drum so that the series of lines *d* would pass behind opening *a*, and series *e*, behind *b*; *c* is a fixation point for the centre of vision. The experiment consisted of two parts:

1st. With the eyes fixated upon *c* and with one of the openings closed, the fastest rate of turning for the drum was found at which the single series could be counted correctly, there being twenty-nine lines in each series, but the subject not knowing the number, the rate being gradually decreased until the correct number could be given with certainty.

2d. With the eyes fixated as before and with both *a* and *b* open, the subject was told to divide his attention between the openings. The fastest rate was then found at which the subject could count the lines in both series at once.

If the attention could really be divided, then it should take but little if any longer for the double series to be counted than for the single. As the attempt to keep separate counts for each series was very confusing, a single count was kept for both. Following is given the number of seconds for the first and second parts of the experiment for each subject.

TABLE XIV.

Subject.	Part 1.	Part 2.
Sd.	3.4	8.9
Hn.	6.6	18.5
Ly.	9.0	26.0
Sh.	12.1	28.0
Hh.	10.4	23.9

This table shows that for all of the subjects it took more than twice as long to count the double series as to count the single. By dividing the time for the first series by 29 we get the time necessary for the attention to change from one line to the other, plus the time necessary for the line to be perceived. For Hn. this was about 0.23 sec., and for Sh. and Hh. a little less than twice this. If we multiply this by the number most commonly given for the objects between which the attention can be divided, 5, we get a little over 1 sec. (1.15) for Hn. and a little over 2 sec. (2.10) for Sh. for the time necessary in Wundt's experiment for the subject to get the number of objects by successive acts of the attention, or the duration of the visual impression. In Experiment A the fastest rate for reading letters was practically the same.

This experiment seems to show that for all practical purposes, at least, the attention cannot be divided. It seems evident that the attention points in only one direction at a time, and then points to but one object, and perhaps to but one perception element of that object at a time. We have to suppose that in cases of animated active thinking the same principle holds in the succession of single images and perception elements that was found to be present in the succession of more complete ideas in Experiment B. There, it will be remembered, the necessary degree of fatigue for a change of attention was lessened by a high state of interest in a secondary direction. In the more microscopical side of the process, a high state of tension in the image centres due to nutritive processes would correspond to the conscious interest on the conscious side. The degree of fatigue necessary for one image to succeed another would thus be made less and the succession made quicker. Fatigue is evidently a prime factor in securing the *succession* element in thinking.

May it not be convenient to establish a unit of mental work along these lines? A single image, however simple, may be too complicated for the basis of such a unit, but the perception element, being the seeming atom of the mental process, would be more suitable. Such a unit might be *the amount of mental work necessary to maintain a perception element in ascendency before the mind one-hundredth of a second*, as such an element could probably not be retained continuously for nearly a second. The amount of effort necessary for this would doubtless vary, but the amount of work would perhaps be constant.

Since in all states of consciousness we must be conscious of something, it is safe to affirm that *direction* is a constant quality of consciousness, and present in all states of attention. I will take the liberty of christening this as one of the *dimensions* of consciousness. There are two other qualities that are also present in all states of consciousness. These are *time* and *intensity*. These two and *direction* I regard as constituting the three *dimensions* of consciousness, or those qualities always necessary to its existence. These deal, of course, with extensive features rather than intensive, looking at it from the outside rather than from the inside. The theories of James and Wundt regard it sufficiently from the standpoint of *direction*, but not enough from that of the other qualities.

I have already dwelt to a considerable extent upon *direction* and *time;* the former distinguishing the object or idea uppermost in the mind at any given instant, and the latter the duration in time units of such ascendency. *Intensity* has here been touched upon only incidentally. It is that which distinguishes a strong feeling from a weak one. In Experiment A it was that which distinguished 'strong revulsion' from 'indisposed to work,' and 'deep interest' from 'indifferent.' In Experiment B it appeared in the power of unusual rapidity in adding; and in the last case of Experiment G, in the beatific aspect of '*A*.' This is expressed more psychologically, perhaps, in the '*Interesse*' of Stumpf, but 'intensity' may be applied also to neural conditions.

These *dimensions* may be combined in a variety of proportions to produce the great variety of mental states well known to all. "The tone of feelings," says Dr. Hyslop, "may be taken as dependent upon the intensity and quality of sensations, *plus* associated ideas." In excitement the intensity is high and time short. In 'vacant' moods the intensity is at a low ebb and the time lengthened. The nature of the mental content with the various feelings, so far as they result from mere associations, is determined by the direction. Momentary changes of the direction to comparatively irrelevant objects—the value of which may be to tell whether they are irrelevant or not—in which the time and intensity are both reduced, seem to make up the 'fringe' of consciousness.

How, then, shall we define attention, and what is its relation to consciousness as a whole? Consciousness more properly is the generic term, while attention is particular. One comprehends a large range of special phases, while the other refers to a single phase composed of the direction, time, and intensity belonging to it, the same as all the kinds of horses we know make up our general idea of a horse, while a certain horse is only one of that large class and has a certain degree of each of the horse qualities. *Attention, then, is a single phase or section of consciousness.* One phase, however, is as much attention as another. It is not one state as opposed to another, but one state as distinct from another. I may be inattentive so far as my inkstand is concerned, but attention exists the same, since my mind is turned to some other object. The process of 'apperception' is going on just the same. The power of holding the mind in a certain general direction is without question of great value, but those who cannot do it should not be deprived of intellect, they simply need training to control this particular dimension. But what shall be said of the adjustment of sense organs in regard to attention? This leads us to a still more theoretical part of the results, the probable neurological basis of the phenomena just studied.

What sort of picture of the brain do these results justify? If we look at a black disc upon a white background, as soon as those parts of the retina stimulated by the white surface begin to tire, rays of light will seem to be emitted from the disc. Soon the white surface around the disc will appear to be illuminated much above the rest. This seems to show that that part of the retina covered by the black color has not been exhausted so much as the surrounding parts, and that a certain amount of energy is allowed to filter out in some form to the more exhausted areas about. Since the retinas, morphologically speaking, are parts of the brain, this suggests that one brain region can assist a more exhausted neighboring region and that neural energy, whether in the form of lymph charged with nutritive elements, or in some more elaborated form, tends to distribute itself equally throughout contiguous tissue. In the brain we have the gray matter composed of an extremely fine interlacement of nerve-cell processes, not only of the nerve cells which lie in that particular gray matter, but also of those in other parts of the nerve centres. The dendritic processes which largely compose this have chiefly a nutritive function, although Ramón y Cajal[1] believes that they may be also conductors of nervous currents between neighboring cells and others at a distance. The chief function of the body of the nerve-cell is also that of nutrition. Nansen[2] believes it has no other function than that of effecting the nutrition of the whole cell and especially of the axis-cylinder process. Schäfer says that while the nutritive function may be the only essential one of the nucleated body, yet there are many cases in which it serves for the transmission of nerve-impulses.

In vision it seems probable that the alternate appearance and disappearance of indistinct objects is brought about by the relation between the supply of energy to the cells innervated by the optic stimulus and the demand for it necessary for continuous vision. This energy is used faster than it is supplied, and there follows in consequence a period of decreased activity due to a degree of exhaustion. The nutritive process, however, continues, and the area stimulated becomes again active through this accession. The exhaustion is again repeated, and so on. The fatigue and recovery of the nerve cells controlling muscular activity would seem to be accomplished in the same way. When, however, the demand for energy is increased the result seems to be different in the two cases. In vision, temperature sensations and probably in hearing, it will be remembered that the stronger the stimulation the more constant was the activity of the sense organ, while in muscular contraction the greater the weight lifted the quicker was the fatigue and the larger the proportion of inactivity. In Experiment E it was found that when the object of attention was complicated or had a special æsthetic value it held the attention longer, while in Experiments A, B and C the fatigue was in direct proportion to the amount of work. Since a lengthening of the time of any special mental activity must in all cases require a proportional increase in the amount of energy consumed, it seems probable that different conditions of nutrition or innervation were to some degree present in the cases of sense stimulation and Experiment E on one side, and muscular contraction and Experiments B and C on the other. In one case the increase of demand increased the supply of energy, while in the other it seemed to have no such effect.

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[1] 'Croonian Lecture,' Roy. Soc., March 8, 1894.

[2] 'Historical Elements of the Central Nervous System,' Bergen, 1887.

I will not attempt a complete explanation of this, but give what to me appears to be the principal cause. In the muscular contraction the demand for energy was greater than the supply; hence, the enforced pause, or decrease in the amount of work done. If the demand had been much less to start with, a gradual increase of it would have been met with a corresponding increase in the supply until the maximum rate in the supply had been reached. With the sense stimulations a maximum state of anabolism was necessary in the nerve cells to make the slightest stimulations become conscious. As the stimulations became stronger, a lower state of anabolism was sufficient to make it conscious, or, in other words, the available supply of energy for the sensation became increased, and more rapidly even than the increase in the stimulus. Our experiment with the black disc shows also that the surrounding areas in the retina assisted in this, and that their assistance was more marked in proportion to the degree of fatigue in the stimulated area. This transmission of energy may be accounted for in one of three ways: By the circulation of nutritive lymph from one area to the other, by the passage of nutritive solution between contiguous tissue by the process of infiltration, or by the transmission of a more elaborated form of nerve energy similar to a nerve impulse or identical with it.

The same reasons for the fatigue effect in muscular contraction would doubtless explain the same phenomenon in Experiments A, B and C, since here the demand for energy was extreme and the variety of function closely restricted. In the case of Experiment E, where æsthetic or associative values greatly increased the amount of energy spent in some one general direction, an explanation must be sought for the supply of so unusual an amount of energy.

In the brain the lymph spaces and the communications between them allow much freedom in the passage of nutritive material from one part to another. There are also reasons for believing local hyperæmia to take place where cortical activity makes special nutritive support necessary. A gradual functional increase in this kind of support due to exercise might explain the increased power of an established function, but would not account for a large amount of support already available, nor for the formation of new relations. Since we have found reason for supposing that each object of attention innervates a more or less restricted locality in the cortex, any object with which associations have been formed might be represented diagrammatically by a centre from which radiate in various directions association fibres relating it with other centres and giving the object its qualities and its position in our conscious experience. Until such relations are established we may regard, according to Spencer and Mercier, the consciousness which such centres excite as feeling rather than cognition.[1] To produce feeling in the various degrees of intensity with which it appears in psychic life, nothing is more evident than that it is evolved at the expense of great nervous energy, and yet it is possible for this to appear with very slight rational articulation to explain its presence. We may see objects or hear tunes that excite a storm of emotion, but which convey little or no suggestion as to the explanation of their effect. In presenting this in the diagrammatic scheme just proposed, we may suppose the radiating association paths to have degenerated so as to be unconscious, but that they still represent lines of nervous energy which concentrate at the supposed centre. This would seem to account for the vague feeling of appreciation accompanied by prolonged attention in Experiment E. Conversely, is it not probable that a demand for nervous force, which is caused by the frequent and persistent retention of one or more mental images, will form new paths of conscious association along the tributary lines of transmitted force thus necessitated? This would imply a tendency on the part of nerve force to diffuse itself through nerve tissue in the direction of localities of low intensity, and this supposition is supported by our knowledge of neurology and our observations of fatigue in the retinas. Nerve cells which reach a high anabolic condition give slight discharges as a result simply of this condition and the general disturbance which the nervous system is continually subjected to, without any specific stimulus being applied to them. As distinct discharges these are largely unconscious, but result chiefly in raising the mental tone. It is naturally the strongest discharge, however, which tends to become conscious, and, if this were in the vicinity of an excited area, there would be a tendency for it to run into it and form a conscious association, especially since cells in the vicinity of such an area would be likely to be discharged by the diffusion of impulses from it.

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[1] Herbert Spencer's 'Principles of Psychology,' Vol. I., Part II., Chapter II. Mercier's 'The Nervous System and the Mind,' Chapter IX.

In connection with this it may be asked whether the familiar phenomenon of association by similarity may not have an explanation along this line. Similarly sounding words or similarly appearing objects might naturally correspond to centres having locations near each other in the brain. Hence, the stimulation in one centre resulting from the presentation of one such word or object would bring the similar word or object into consciousness through the radiation of nervous disturbance from the first centre to its neighboring one. The confusion between like sounding words so often noticed in children might be explained by this, and by the lack of fixed paths of association which become more firmly established with increased maturity.

It is well known that as a cortical region comes to have a distinct function, its nerve processes become more developed and the axis-cylinders become medullated. We also know that as an object of attention comes to figure more and more in consciousness, its qualities come to be recognized and its relations more definite. Its simple *thingness* disappears and gives place to qualities that are regarded as belonging to it and which come up with it from association. This is certainly evidence that definite neural relations have been established. Feeling thus seems to organize itself into cognition and distinct thought as neurons take on specific functions by means of extension and medullation. From the fact that nerve-elements do not proliferate after birth the development of brain organization can not depend upon this. "On the other hand," says Dr. Hyslop, "it is probable that, in those regions which are most exercised, mental activity involves a greater development of the protoplasmic apparatus, and of the system of collateral nervous paths."[1]

The selective power of inhibition is evidently the greatest factor in bringing about this organization. The irrelevant ideas are by this means suppressed and the more promising ones allowed to establish firm and lasting relations. It is, doubtless, this kind of development which allows the philosopher or crank to see everything in the light of his pet theory, and which gives to each of us our characteristic habits of thought. It is the lack of hard continuous thinking, says Hirth, which makes the mind so subject to the disorganization found in the cases of distraction in insanity. The lack of this discipline keeps our mental possessions in the state of a mosaic instead of making them into a continuous harmonious unit.[2]

This tendency of continued mental effort to cause relations to be formed between different parts of the cortex, and perhaps between very widely different parts, is a factor opposed to what we may regard as the mere natural grouping of related ideational centres by themselves. This grouping—which would evidently depend more upon the permeability of the paths of intercommunication than on close spatial relations in the cortex—must have been the cause of the isolation of the centres for different mental functions found in Experiments E and F. This natural grouping would seem to result from the qualities and associations which objects most readily present to us, while the more laboriously formed paths correspond to logical relations. In Fig. 11 this is expressed diagrammatically. The association path between those centres corresponding to the perception elements of the same object are more numerous than those between different general objects of attention that are less frequently associated (as *h*, *i* in Fig. 11). The paths relating the general groups of ideas *e* and *m* are less direct and less numerous than those between *i* and *e* because they are less frequently united by experience.

When many such paths representing logical relations have been formed we may suppose that energy in one part of the brain is more easily brought to the support of an exhausted centre than before. "When," says Mercier, "an entirely new

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[1] 'Mental Physiology,' p. 54.

[2] G. Hirth, 'Lokalisation Psychologie.'

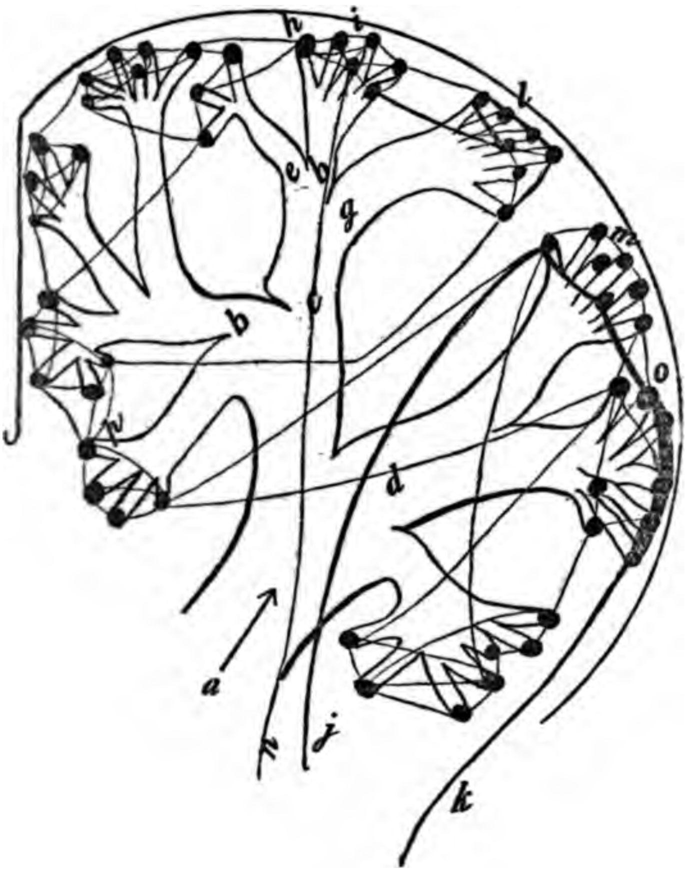

FIG. 11.

Let the accompanying diagram represent a portion of one side of the cortex. The large trunk beginning at *a* represents the channel of nutrition for the parts above; *b*, *c*, *d* represent the branches of this trunk that lead to areas representing larger groups of ideas, like home, occupation, amusements, etc.; *e*, *f* and *g* represent the divisions of such a general group, corresponding to the various instruments of occupation, etc., and the round dots like *h*, *i* the seat of conscious mental activity and the areas that correspond to the simplest perceptive elements of objects. The finer lines that connect these represent association fibres; *j* is the sensory tract and *k* the motor tract for a reflex movement. The association paths connecting the centres involved in this circuit are represented as being so enlarged as nearly to obliterate the existence of different cen as separate steps in the process.

relation is established in the mind—when we have an entirely novel thought—then there is the passage of a current through a previously untraversed portion of ground-substance—through a virgin soil. The passage of this current rearranges some of the molecules into polar parallelism, so that subsequent currents pass more easily."[1]

Let us take a moment to trace the possible development of an association path, formed in the more easy and ordinary way. A sensation is received which has never before been experienced. As such it creates a tension in the cortex through the liberation of nervous energy coming from the ganglion cells affected, but has no meaning for the person until related to other experience by means of the confluence of this nervous tension with others. Let us suppose, however, that it does find an idea present in the form of similar tensions in other nerve cells. Now whether we consider these tensions to be caused by electro-motive force, by some system of hydraulics, or by molecular vibrations, it would be natural for them to spread in conductive tissue, and hence to travel towards each other. When this union has been accomplished, a new association path has been opened up, and in future the sensation which was without meaning will be related to other experience. It will, in the future, be brought in whenever the ideas associated with it reoccur, or it may be the means of bringing them in. The more this chain of associations is used the more certain is one part to bring up the others connected with it. The conscious experience which corresponds to this process is the line of association, every step of which is at first consciously taken, but when repeated becomes passed over more and more quickly, until finally the whole line appears as but one association. It is in this way that general ideas are formed. My idea of a horse, for instance, is made up of a multitude of once conscious associations, but which now are forgotten. They are all present, however, though unconsciously so, in the fusion which gives the general idea.

There are, of course, all degrees of fusion according to the frequency or number of past associative acts for any particular line. The process has not gone far when we recognize, as we so often can, the slow accumulation of energy from various ideational centres as the force of many reasons for some motor accomplishment. It is the energy combined in this way which gives the dynamic force or impetus to acts of volition. Automatic acts may result from a single idea, but the voluntary act appears usually as the conclusion to a syllogism whose premises are at least two antecedent connected ideas.

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[1] *Loc. cit.*, p. 370.

It is to the association which has become unconscious that we must attribute sense organ adjustment and its relation to attention. No doubt, at first the turning of the eyes, poising of the head, and even the adjustment of accommodation and adaptation were once conscious, and made with special effort to put the sense organs in accord with a mental image. We have only, however, to suppose the paths of association between the visual image and the organ of vision to become enlarged by use, when the related motor mechanism would cause adjustment by the mere tension of liberated energy in the visual centres. In the experimental work of Dr. Heinrich, already referred to, it was found that visual images caused a disturbance of the pupil of the eye, while other images did not. Evidently the movements of adaptation are so immediately under the control of the visual centres that the power of voluntary adjustment has been lost.

It may be well to draw in more general terms the outlines of the picture which nutritive processes in the brain present. For convenience of presentation we may suppose the cortex to be divided into general areas which roughly correspond to general directions of attention. Thus, the mental images and associations which I have in regard to my home may have their origin in one such area, while those in regard to my occupation are largely confined to another; and so on, each general class of ideas depending upon its own particular location. Relations may, of course, be formed between different areas, but since we may suppose the internal relations between different parts of such an area to be more numerous than their relations with the parts of any other area we have a general basis of division. We will suppose these larger areas to be made up of smaller ones which correspond to the details of the general objects of attention, as the rooms and furnishings of my home, or the different books used in my occupation. Furthermore, we will suppose each single object, such as room and book, to be make up of various features, the final elements of perception, each of which has its corresponding area within this area. The source of nutrition for each of these smallest areas we will suppose to be from the general fund of the next larger which contains it, and so on, the same as each twig of a tree depends upon the branch, and the branch upon the trunk; but that cross supportive relations, as from twig to twig, are also present, though more difficult and of less degree. These last would correspond to association fibres. (See Fig. 11.)

Each of the largest areas would have the only means for the outlet of its energy through the activity of the smallest that compose it. Wherever the tension of escaping energy would be greatest, there, for the moment, would be the seat of consciousness. The content of consciousness, whether my pen or a page number, would be determined by the locality of the activity; and the order of these localities in their turn of discharge would be controlled partly by the association fibres, which, as they become larger and larger by use, would be an increasing factor in this control, and partly by the degree of anabolism in the various cortical centres. Without this influence of association fibres each cell would have a regular rhythm of anabolism and discharge, varied only by the nutritive supply.

Bevan Lewis says, "the cell is subject to a constant supply of nutritive plasma, it gradually assumes a state of nutritive instability, and will necessarily discharge its accumulated energy in accordance with the simple law of nutritive rhythm, the resulting stable equilibrium is succeeded by a measurable period ere the potential energizing of the cell has once more brought it up to its former state of instability. Were this all that occurs the process of storage and liberation of energy would be a simpler rhythmic process than the more compound rhythm which actually pertains to mental operations."[1]

Granting a rhythmic tendency to the cell as influencing mental activity, it is certainly natural, from a purely physiological

[1] Bevan Lewis, 'Mental Diseases,' p. 110.

point of view, to expect closely related groups of cells to have rhythms for the same reasons, and this is precisely what seems to have been observed in all the experiments described. While the author just quoted believes the nucleus to be the chief factor in regulating the cell discharge, it seems to me that Mercier[1] is more correct in regarding this to be due to the influence of cell fibres. These, we may suppose, conduct enough additional energy to the already anabolized centre to make the intensity of its discharge greater than that of any other in the cortex for the moment, and thus have a controlling influence in determining the mental content.

We certainly also have mental phenomena which indicate similar relations to exist between the larger areas. A sufficiently strong impulse coming through association paths may augment the energy of a fatigued or dormant area sufficiently to cause action, when otherwise it would be impossible. By this means remote motives may come to control conduct, or one's life may be raised above the control of merely temporary impulses. A well-known writer has said that good acting and good morals are synonymous. Let a 'dirty foreigner' brush against a lady's dress on the street and she is filled with disgust, but let the feeling of pity be brought into her sensory motor arc, as by the offender's falling into a faint, and the motor accompaniments of a very different attitude are adopted. In Experiment G it will be remembered that a strong positive feeling for relatives was induced by the news of sickness. It is a frequent situation in fiction for a young lady's attractiveness to be greatly enhanced when found to be possessed of wealth. A sufficient inducement will make the worst of bores tolerable.

This may be expressed in Fig. 11 by supposing the circuit n, i, l, p to discharge through k, when the circuit is usually confined to j, m, o, k.

We need not suppose, however, that there is only one line of neural associations at once. The more or less automatic processes, the snatches of rhythm, the humming of the popular tune, now and then break into the more conscious lines of mental activity, even without the aid of sense disturbance, to bear testimony to their existence. What we get in consciousness is primarily a succession of highest intensities, whether they are connected by associations or not. In Experiment B it will be remembered that the fatigue of the adding functions was often shown by the appearance of thoughts which were not even suggested by the work. Yet there usually appears a certain unity and method in the progress of thought which suggests the subordination of many centres to the one line of associations discharging. As an illustration may be cited the inhibitive effect of a strong sensation, or an unusually clear perceptive state, as in surprise. Movements, especially of an automatic nature, cease; the sense organs become adjusted in accord with the sensory idea, and all unrelated activity, mental as well as physical, seems paralyzed. It may be that there is some immediate reduction of intensity in other cortical centres in favor of the one excited that causes this state of inequality, besides the direct increase in the excited centre from the stimulation. If this is so, the chances for an equal intensity existing in different centres at the same time would be greatly decreased. There are, however, times when this condition does seem to exist, as when many distracting inclinations cause momentary confusion, or when indecision comes from the balancing of reasons for and against some line of conduct. The ass starving between two bundles of hay is not a frequent spectacle, however, and it may be that natural selection has provided us with some neutral arrangement to avoid this.

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[1] 'Nervous System and the Mind,' p. 73.

The theory of Ramón y Cajal is in accord with this. He believes that when a sensation or idea excites a certain region of the cortex, the perivascular neuroglia cells cause hyperæmia in that locality of nutritive-lymph, thus supplying energy (evidently at the expense of surrounding areas) for the increased intensity of mental action.[1]

The work of Roy and Sherrington leads them to conclude that "the chemical products of cerebral metabolism contained in the lymph which bathes the walls of the arterioles of the brain can cause variations of the calibre of the cerebral vessels; that in this reaction the brain possesses an intrinsic mechanism by which its vascular supply can be varied locally in correspondence with local variations of functional activity."[1]

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[1] See His u. Braune, Archiv für Anat., 1895, p. 377.

This activity may, however, be stimulated to an unhygienic degree. Over-exertion of the brain may lead to acute mania, melancholia, dementia, *folie circulaire*, and general paralysis.[2] Even without marked intellectual or moral weakness, even in brilliant mental accomplishments, says Professor Flechsig,[3] many persons show brain weakness in the nature or inequality of their behavior. Dr. Tissié[4] finds that with sane and robust persons extreme physical fatigue, as that caused by a long march or bicycle trip, or any regular prolonged rhythmic muscular work, excites distinct temporary psychoses which have the same outer manifestations as the pathological psychoses of morbid subjects. Among these are loss of volition, pholias, hallucinations, amnesia, hypermnesia, automatisms, and a well manifested state of suggestibility. These same psychoses appear in the insane, only more readily than in the healthy person.

Morbid subjects of this kind, and those living in extreme old age, in whom the psychic life centres about old and deep-seated associations, seem to be victims of chronic fatigue. Nerve cells found in these subjects stain more deeply than the unstimulated cell.[5] If stimulation is continued in healthy subjects for a long time the protoplasm becomes deformed and crenated. Finally a state of complete collapse is reached in which the nucleus and protoplasm lose all power of taking on stains. Dr. Tuke gives us the following phases as they result from over-exertion:

First there is excited an increased blood supply, "the consequent dilation of the vessels is maintained by the lessened alkalinity of the cerebro-spinal fluid, and the discharge of energy of the cells becomes irregular in consequence of the presence of more blood than is needed for repair, and the discharge takes place at a lower level of cell nutrition and function." A sult we get in order active hyperæmia, passive hyperæ congestion; leucocytes are deposited, indicating a state flammation; red corpuscles in various stages of decay an masses of pigment are deposited, and proliferation of fixe nective-tissue cells and exudation occur.

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[1] *Journal of Physiology*, Vol. XI.

[2] See J. Batty Tuke, 'The Insanity of Over-exertion of the Brain.'

[3] *Op. cit.*, p. 32.

[4] Ph. Tissié, "La fatigue chez les débiles nervoux ou 'fatigues'," *Revue Scientifique*, November 21, 1896.

[5] See Dr. C. F. Hodge, 'Changes in Ganglion Cells from Birth to Senile Death,' *Journal of Phys.*, XVII., Nos. 1 and 2. J. Batty Tuke, *op. cit.*, p. 22.

In optical phenomena we have positive or negative images according as to whether those parts of the retina cerned have been stimulated only enough to start an a which continues to liberate energy, or have been stimula exhaustion. So in mental activity we have, on the one vigorous action and positive feeling in response to a s tion, thus indicating a high anabolic condition; while, other hand, we have negative feeling and either delayed relevant activity, indicating a low catabolic condition. Re day-dreams, involuntary thought in general, and esp dreams while sleeping, take on these after-image cha istics. Mr. Julius Nelson[1] states that in his youth he wa ject to dreams in which were repeated certain frightful s He found, however, that these could be prevented. I wi his own words.

"My mother taught me a remedy for bad dreams w applied with immediate and universal success, viz., on c ing myself for sleep, the object of a dreaded dream w voluntary act, brought before my mind, and while held t said mentally, 'Shall I dream of that?' (here visualizi scene which past dreams had taught me to fear) and th subject was dismissed with a confidence that I should troubled by that dream for that night. Should the feared again intrude before sleep came, it must once more be dis by the formula or my work was in vain. Thus, one by rehearsed the list of bugbears every night, making effort not to treat pleasant subjects in that way, for then I they would not be dreamed about."

I have myself successfully followed the same princ exhausting certain ideational centres to prevent them troubling me in sleep; mental activity irrelevant to the hausted by this exercise evidently following from this principle of negative mental after-images.

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[1] *American Journal of Psychology*, May, 1888.

As illustrated by Experiments A, B, C, etc., the first effect fatigue is inequality in the amount and quality of work. xperiments E and G showed how when this exhaustion is balnced, or when an effort is made to balance it by special exerion, a negative feeling is induced in connection with the object of attention. When this exertion passes the bounds of safety the milder forms of insanity spoken of by Dr. Tuke and Professor Flechsig seem to come as the result. What I wish to call especial attention to is the negative feeling. This is also excited in regard to objects which have harmed us or caused pain. Fear is a typical form. Experiment G has doubtless served to show how broadly negative feelings figure in the lives of all of us. With some it becomes chronic, as when in the 'blues' or melancholia it indicates a general low state of vital energy. It is probably due to irradiations of this feeling which give to things which in themselves have no emotional tone a 'tinge of sadness,' such as the moanings of the ocean and the sighings of the pines.

In consideration of the relation of negative feeling to fatigue and resulting insanity, we seem justified in regarding it as a natural means for self-preservation. Evidently it comes as a warning, which, if disregarded, is prevented from acting as it is intended to in keeping us from insanity, or at least from a state of exhaustion which would delay the ordinary rate of recuperation, or lay us liable to disease. When it appears as fear or dislike for a person or situation, we instinctively recognize its value and profit by its signal. When it appears as a sign of ver-exertion, evidently it should no less be heeded. This suggests that there is yet to be preached a gospel of scientific receation.

On the other hand, a positive feeling seems to indicate a high nabolic condition. While the failing of vital force is indicated, s in the aged, by a decrease of physical activity and disconection of thoughts, one general direction of attention changing another without the usual steps of association, a surplus of ergy is evidenced by vigorous mental action and a healthy exercise of the imagination. It is this state which makes the waters laugh, the breezes play, and which gives the optimist his sanguine hopes. Now the objects of attention bloom out with attractions never before seen, and cold logic cannot say they are lacking. Now is seen 'Helen's beauty in a brow of Egypt,' and the strongest impulses to beautify and to self-expression appear as bubblings over of this energy. "Deep and tenacious cases of spontaneous attention," says Ribot, "have all the characteristics of unassuaged passion which unceasingly recommences in the effort to satisfy itself."[1] With a strong and menacing negative feeling in one direction, which may emphasize a still stronger positive feeling in another, is it any wonder that many persons often overstep the bounds of propriety and find themselves branded as aliens? And does this reflect more upon the personal morals of such than upon customs which insufficiently recognize the needs of our common nature?

There is also a large class of sensitively conscientious people who afflict themselves with reproaches or torturing self-examinations, because the emotional strength which once gave them a constancy of feeling has been diminished by ill health, made unconscious through more perfect cerebral organization, or else exhausted by over-exertion. Such should be taught the basic principles which underlie our mental constitution; and that mental hygiene is as imperative as physical hygiene, the same general principles applying to both.[2]

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[1] Psychologie de l' Attention,' p. 17.

[2] Perhaps the most severe criticism to be placed upon the present treatment is that there is the proportion of two of theory to one of fact; the ratio more properly being inverted. If, however, the experiments described help to indicate a fruitful region of experimentation, and one in advance of the classical reaction-time experiments, their end will largely have been realized. But even with such limited data one is impressed with the common conviction that facts are greater than theories. Any theory, therefore, advanced in the present state of psychology can hardly be expected to serve more than as a suggestion for a more complete one.

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Experimentally and Subjectively Considered

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CONTENTS.

1. Introduction:
    (*a*) Mental imagery compared with imagination 2
    (*b*) " " " " sensation 2
    (*c*) " " " " visual after-images . . 2
    (*d*) " " " " memory-images . . . 3
2. Variety of types of mental imagery 4
3. Research and experiment: 5
    Subjective methods 5
    Objective methods. 6
        Experiment *A* 6
        Experiment *B* 16
        Experiment *C* 24
        Experiment *D* 29
        Experiment *E* 32
4. Illustrations of types of imagery in myself 36
5. Word imagery 40
    Slips of the tongue and pen 41
    Analysis of my word imagery 42
6. Historical note on the psychological description of mental imagery 47
7. Supplementary 54

MENTAL IMAGERY.

INTRODUCTION.

Under mental imagery I wish to place not only the images of the so-called five senses, but all the mental representations that are, in part or in entirety, the reflection or 'echo' of the world of sensations. And I would have sensations here include also all the 'internal' sensations and the sensational elements of pleasure and pain.

The terms 'reflection' and 'echo' are but figures of rhetoric and are misleading if taken to mean nothing more than the faint reflection or reproduction, after an interval of time, of something that has happened to us. For in the case of what I shall presently mention under the name of mental imagery of emotion, there is no reason to pre-suppose any individual love, hate or angry feeling of which the emotional mental image is a reproduction. It is a part of the stream of thought, and is to be so regarded in just the same way that visual or auditory or any other imagery is a part of it.

Mental imagery might finally be defined as the content of our mind when we have no sensation but yet are not asleep; when we are alone, in the dark, in complete silence, and oblivious of the chair or bed or bit of ground on which we happen to be sitting, lying or standing.

In this essay no attention will be paid to memory except when it *is bringing* before the mind the sensations of the past. It is thus an essay on one aspect of memory. Neither will attention be paid to laws of association. Associations have been collected by psychologists from all sides but (as an association is no more than the manner in which thoughts come to the mind, as it is an observed order of events and not an event, the event being the mental imagery) they will not be noticed *qua* associations at all.

I. *Mental Imagery Distinguished from Imagination.* By imagination is here meant the 'faculty' generally called, more specifically, creative imagination. It is that which makes great works of art, whether they be paintings, sculptures, poems, symphonies or cathedrals. The possession of the creative imagination implies that of mental imagery, but not *vice versa*. Imagination is something abstract and indescribable; imagery is concrete and experienced by every one. Imagination is something that cannot be itself represented in mental imagery save by a feeling; mental imageries are on the other hand quite as real (not objective, however) as sensations themselves and play quite as important a rôle in our lives.

The association in our minds of the creative imagination with mental imagery is somewhat far fetched from the real nature of things, and is the result of the similarity and like etymology of the English words which are used for these two aspects of mental life. So with imagination as a creative faculty I have nothing to do in this essay.

II. *Mental Imagery Distinguished from Sensation.* It is almost, if not quite, impossible to draw a line between mental imagery and sensation, as any treatise on illusions and hallucinations will amply demonstrate. On the one hand, illusion becomes so faint as hardly to be known from mental imagery, and on the other, mental imagery is sometimes so strong as to become an illusion. We might, however, make a verbal description limiting mental imagery to those 'subjective sensations' which leave us in no doubt as to their origin, whether internal or external. Where we are in doubt we say it is an illusion.

III. *Visual Mental Imagery Distinguished from Visual After-Images.* Fechner's well-known distinctions may be summed up as follows: The visual after-images are exact copies (in shape) of the originals; seem unsubstantial, though sharp in outline; *feel coercive; cannot be voluntarily changed;* are continuously enduring; are bright but almost colorless; are more easily observed with closed than with open eyes; *seem to move with the eyes;* and have no perspective, being, when the eyes are closed, in a dark, flat and contracted field. The attention seems to be directed *forward* toward them. The mental imagery, however, is the direct opposite to this in all ways, according to Fechner. My own repeated experience agrees with his only in the statements which I have italicized. The after-images feel coercive etc., but not more so than do the mental imageries (see pp. 53 and 59). I find also that my attention has no forward or backward in purely visual terms. All my mental imageries are conceived as *before* my eyes to be sure, but this before has no *behind.* When visually imagining what is behind me, I always manage to see it before my mind's eye. In other words I see no direction in mental imagery. I can not compare mental imagery and after-images in point of substantiality. They seem to be totally incommensurable.

To me the after-images seem blurred always, there being no sharpness of outline at all. The mental imageries are, on the other hand, sharp in outline, but only in one spot at a time. Otherwise they are as indefinite and incomplete as is actual vision (when one looks at a thing, as one does every day, many times, and does not see it). I never had a visual after-image so large that any part of it was out of the focus of attention when I was looking at any other part of it.

After-images of sensations other than sight have not been adequately recognized in psychological literature. They are in many ways so hard to distinguish from mental imagery that their appearances have never furnished an easy field for investigation. I believe that I have observed in myself what might be called tactual after-images.

If I have auditory after-images it is absolutely impossible to recognize them. It might be that the occasional recurrence of a melody after a concert or an opera, is an auditory after-image, but it has none of the objectivity and constancy of the visual after-images and can no more be compared to them in intensity than can visual mental image be compared with visual after-images.

IV. *Mental Images are not Memory Images.* The frequently recurring phenomena, memory images, though they stand about half way between, seem to be nearer to after-images than to mental imagery. I am unable to recall any memory images that have any of the coercive quality of after-images.

They are usually completely dispelled by a contradictory sensation, *e. g.*, the memory image of a pen behind the ear is completely annihilated by putting the hand up and letting the fingers feel that it is not there.

I have sometimes a very strong feeling which seems to be a memory image and yet has otherwise all the marks of a mental image. If a person comes into the room unseen and I hear it, I have a feeling of direction, of approach, of vicinity, of a physical body near (How can it be expressed?), at any rate a definite feeling as if, perhaps, the nerves in that part of my body towards the person were pointing in the direction of the person. All this, though I have not looked around, and there is *no visualisation in it*. Then, if the person leaves the room, the indescribable feeling still continues. As I have observed it in myself it has *always* continued until I have noticed that there was nobody in the room but myself. Then it completely vanishes. It is a rather unpleasant feeling all through, and if I were not too absorbed I would immediately dispel it by looking around. Sometimes it has lasted for a minute or two.

But whether the image be a memory image or mental imagery seems to depend on our subsequent recognition that the former had an objective stimulus. In such case then the distinction would in this essay be immaterial.

The Variety of Types of Mental Imagery. We have mental imagery from all the senses; that is, some of us are conscious of it. Nobody except the born blind and deaf would deny the existence of visual and auditory mental imagery. Many persons claim to be able to recall the smell and the taste of fruit. A few would recall the "*touch* of the vanished hand," but I think very few are conscious of all the mental imageries enumerated in the following table, viz.:

1. Visual.
2. Auditory.
3. Tactile.
4. Gustatory.
5. Olfactory.

Kines-aesthetic

6. Thermal.
7. Motor.
8. Those of pain (not touch).
9. Organic.
10. Those of emotion.

These mental imageries have the specific qualities of the senses to which they correspond. To recall the *appearance* is an entirely different experience from recalling the *taste* of an apple.[1] To recall the *touch* of a piece of ice (wet, slippery) is an entirely different image from recalling the temperature (cold) or the appearance of it (white, glistening). The various mental imageries are different also in the amount of pleasure they give (both intensity and extensity). The pleasure of dwelling in the imagination upon a glass of wine or a delectable sweetmeat is of very short duration. I believe it is shorter if it be the taste and smell of it which is contemplated than if it be the look of it.

RESEARCH AND EXPERIMENT IN THE FIELD OF MENTAL IMAGERY.

The pure introspection with which each one examines his own mind for the sense elements therein might be called when the results are recorded and tabulated, the subjective method of investigation. In my own case I study the mental imagery in myself and isolate it as much as possible from the outside world. This seems to be the only way to study mental states at first hand and as such will become more and more important as psychologists appreciate more and more the inevitability and authority of direct introspection.

To show how difficult it is, on the other hand, to be absolutely sure whether one's introspection is unconsciously deceived by the knowledge of relations, I would note that one of my mistakes in learning stenography seemed to indicate a latent visualization. Thus in writing the word for apology I wrote an outline ending with ━ = gy. To the eye, therefore, this outline is, to all intents and purposes, correct, if I suppose myself to have visualized the word 'apology' in Roman type, but ━ represents the hard sound of *g*, the soft sound of *g* being given by /. So that my inference was that, though I was conscious of mentally hearing the word 'apology' correctly pronounced in my own voice, yet I wrote from a visual rather

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[1] The sight of a red astrachan used to make my mouth water and cause a sharp and delicious pain in my jaws. It doesn't now, but when a tart apple is suggested and I dwell upon the purely sensuous side of it my mouth *seems to feel as if it were watering.*

than an auditory image. It might be argued that what I must have associated with the horizontal heavy stroke while writing the word was the look of '*g*' with which the former is correctly associated and not the sound of '——' which is dissimilar to the sound which I heard in my mind's ear. But though the association was in this case most natural, it was not conscious. I had no visual imagery at the time.

I would call *objective* methods all questions, bearing upon mental imagery, given by myself to others, both in writing and orally, such as the well-known questions of Galton. Questions I would call direct or indirect as they do or do not ask the subject directly to analyze his own thought stream. The value of all objective methods seems to be less than that of the purely subjective and individual one, as questions are often misunderstood and thoughtlessly answered. Thus such questions as "Can you recall sensations of taste, smell, etc?" have been answered in the affirmative but explained in such a way as to throw doubt on the answer. One of my subjects said he recalled the *crisp* taste of celery.

The direct question "Have you good visual auditory, mental imagery, etc.?" assumes that the answer is to be given by a trained observer of mental phenomena; and yet it must be put to every one.

The indirect questions "Is there a VI on the face of your watch?" "Which shoe do you put on first?" "How do you fold your hands?" etc., when the subject answers without looking at the objects in question, would seem to help us to infer whether the mental imagery is strong or not and even to what sense it belongs. From the answer to the question about the face of the watch we infer, if the answer is correct, that the person has a good visual imagery, and by the question "How do you fold your hands?" we might elicit whether or not the motor or tactile imagery is good.

Experiment A.

Among these indirect questions is also included the experiment which I have made upon 100 juniors (2 classes, '96 and '99) in Columbia College. I have read aloud to them a short passage (translation) from a French novel, which took about a minute to read and which, as I believe, contained words likely to arouse in the minds of the hearers the mental images most noticeable in our every day life, *i. e.*, those of sight and sound. The words aroused in me a certain number of mental images which I could specify and record, and I supposed that others would have the same aroused. This, of course, does not necessarily follow but it seems likely. Then the juniors were asked to write down all they could remember of the passage, whether in the same words or not. From the number of words written down by them which (to me) suggested mental images I compiled a table which showed what per cent. of visual and auditory images had been recalled. This supposed that what was recalled would be the easiest and that the easiest to remember for each one would be that which most corresponded with the tendency of his mind. If he was eye-minded he would naturally remember the visual elements of the description; if he was ear-minded he would naturally recall the auditory elements. This experiment I call an indirect question, because it attempted, by means of the memory, though hampered by the memory's imperfections, to ascertain whether the mind worked in terms of one sense more than another and, if so, how much more. Answers to this particular question were quite satisfactory in some ways. The results were large. A hundred answers to it could be gotten in 15 minutes; and the matter could be very easily tabulated and a statement made with some show of authority that "You have 76% visual and 20% auditory" or "You have 80% visual and 15% auditory."

But though indirect questions are likely to be better understood, and are more to be relied upon than the direct questions, I think that the experiment just described, and indirect questions like it, are open to several important objections. In the first place, in a given number of words there is not a definite and constant number of visual or other images, because the imagery belongs not to the word so much as to the person, and I myself might have more or less than others. In the second place, in an experiment which involves the memory even for the space of one minute, a complete register of the mental imagery of each student is hardly to be expected.

As a test for the mental imagery I gave the same blank to students (52) that I gave to artists (p. 16) and supplemented the blank in the case of students (100) by the following. I said: "I will now read to you a translation from the French of Flaubert, after which I wish you would write out as much of it as you remember, and then try to describe how you remembered it, whether by the words or by the visual or other images that are called up:

> "She always went with him to the first step of the outside stairs. If his horse had not yet been brought forward they remained there. They said good-bye; they attempted no further remark; the fresh air encompassed her, played with the downy hair of her neck or blew about her side the strings of her apron which twisted and twirled like a little flag. Once, when a thaw had set in, the water was trickling down from the bark of the trees and the snow was melting from the roofs of the buildings. She stood on the threshold; she went back to get her parasol; she opened it. The parasol was of a changeable green and blue silk and the sun shining through it lent a radiant and flickering lustre to her white complexion. She smiled beneath it while the soft zephyrs played about her and the raindrops were heard to come pattering down one by one on the outstretched silk of the parasol."—*Madame Bovary.*

They were allowed ten minutes to write what they remembered of this, after which I read to them an extract from Bain, making the same requests as before concerning it:

> "The subject-matter of geometry embodies a few fundamental notions and processes. A definition, an axiom, a postulate, a proposition, whether theorem or problem, a chain of demonstration, are to the beginner things absolutely new; they must be fixed by the plastic power of contiguity; and time and concentration must be allowed for the purpose. But in a good head one or two examples of each, strongly imprinted, will make the rest easy; the method or character of the devices will be seen through and acquired and in every new case the mind will fall back upon the old ones for the common element, and concentrate attention upon the points of difference solely. When, after going over a few definitions, the learner is impressed with the form and peculiarity of a definition, there is little to acquire in the rest: a slight substitution serves to make a new one out of an old; the definition of a square is easily changed to suit a rectangle."

It is to be noted here that these two extracts differ one from the other in the following respects:

In the first there are 166 words only 72 of which are words that arouse imagery from one of the 'five senses.' There are two[1] such words occurring twice and one[2] that occurs three times.

There are 7 unusual words: *encompassed*, *twirled*, *trickle*, *radiant*, *flickering*, *lustre*, *zephyrs*.

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[1] Play, silk.

[2] Parasol.

There are 13 words with few (or no) synonyms, viz, *went*, *stairs*, *horse*, *apron*, *strings*, *flag*, *thaw*, *snow*, *green*, *blue*, *silk*, *sun*, *smiled.*

In the second there are 165 words, only 70 of which arouse imagery from one of the 'five senses.' There are six such words occurring twice (*concentrate*, *few*, *acquire*, *rest*, *new*, *old*) and one which occurs four times (*definition*). Two have words so nearly like them that the image would be the same (concentrate=concentration; impressed=imprinted).

There are two unusual words: *plastic*, *contiguity*.

There are six words with few (or no) synonyms; *axiom*, *postulate*, *new*, *old*, *definition*, *square*.

In the first extract the word raindrops is misleading. It is evidently a slip of the translator and it resulted in a large per cent. of the subjects affirming that it was raining and five per cent. using the word 'umbrella' instead of 'parasol.'

The first extract can be arranged into eleven periods, each of which may be regarded as a visual (or auditory) unit; and each one taking as long to read aloud (in my own case, at any rate) as a visual mental image naturally and usually remains before the mind's eye. There is also some change in the picture for each new period.

The second extract seems to proceed in like manner step by step, and to be divisible into ten periods, at the end of each of which one can pause with the feeling of a step taken. Possibly it is this feeling which is most frequent in reading a book on any abstract subject. As I read the quotation from Bain I go through a sort of undulation (so to speak) of feeling. At the beginning of the clause I am straining my attention, and at or near the end of it, having 'grasped the meaning' of the author, an immediate feeling (*A*) of "Yes, that is so," or (*B*) "No, it isn't," supervenes, and I go on to the next sentence. Sometimes the "Yes, that is so" feeling is so strong that I am moved to underscore the phrase or score the margin of the page, or if the "No, it isn't," feeling, that it is inconsistent with itself or with my own experience of life, is very strong, I write out the clause on a piece of paper, in order to get the feeling of (*C*) 'reviewed' or 'passed muster.' If I do not, I have the feeling of (*D*) 'omission,' and am not satisfied with my perusal. In this manner, as I read the book or the paper the feelings *A*, *B*, and *C* or *D* continue to follow one another.

None of the periods of the extract from Bain is capable of being represented by mental imagery from the five senses, in any form (visual, etc.) but is represented or accompanied by feelings which are similar to mental imagery in their faintness and obscurity. The words of the extract are represented in my mind only by the mental imagery of them *qua* words heard, and by the faint feelings just noted, which I should like to call mental images.

The accompanying Table I. shows in column I. the 72 words or phrases into which I have divided the first extract. Each of these, I infer, contains one *idea* which either is a mental image (from one of the 'five senses') or is a feeling of relation, and in the memory is revived (as one or the other of these) before being put into words. In column II. are given the numbers (out of 100) of the words correctly reproduced by the subject. In column III. are the *ideas* correctly remembered but expressed in words different from those of the extract. In column IV. are the total number of ideas correctly reproduced. *E. g.*, for phrase 43, 70 men out of the 100 wrote parasol, 4 wrote umbrella, thus 74 remembered the *idea;* and 26 said nothing about any kind of protection from either sun or rain.

In the V. column the totals for the eleven groups are given, and in column VI. are given the per cent. of the groups. Thus in group (*a*) 333 ideas were reproduced out of a possible 600, or, as column VI. shows, 55.5%. Column VI. shows also that the group which was best remembered was, however, group (*b*), 'If his horse,' etc., and that next to it came group (*g*), "She went back to get her parasol"—both short sentences—after which came group (*i*), "The parasol was changeable," etc. It is interesting to note, however, that the per cent. of the words "The parasol was changeable green and blue silk, and the sun shining," would be 49%. So that it may be said that all that was remembered by about half the men was "She always went with him to the first step of the outside stairs. If his horse had not been brought forward they remained there. She went back to get

FLAUBERT. BAIN.

	I	II	III	IV	V	VI		VII	VIII	IX	X	XI	XII
		Words and Ideas.	Ideas only.	Total.	Group Totals.	%			Words and Ideas.	Ideas only.	Total.	Group Totals.	%
(a)	1 she went	64	11	75			(l)	1 the subject matter	18	1	19		
	2 always	54	6	60	(a)			2 of geometry	79	1	80		
	3 with him	53	8	61				3 embodies	16	11	27	(l)	
	4 to the first steps	47	18	65	333	55·5		4 a few	9	1	10		
	5 of the stairs	35	2	37				5 fundamental	29	1	30	179	25·5
	6 outside	31	4	35				6 notions	3	1	4		
(b)	7 if his horse	69	2	71	(b)			7 and processes	11	2	13		
	8 brought forward	32	18	50	175	58·3	(m)	8 a definition	49	0	49		
	9 they remained	22	32	54				9 an axiom	75	0	75		
(c)	10 they said good bye	44	3	47				10 a postulate	60	1	61		
	11 they attempted	10	3	13	(c)			11 a proposition	32	0	32		
	12 no further	11	26	37	110	27·5		12 theorem	25	0	25	(m)	
	13 remark	8	5	13				13 problem	7	0	7	310	31·0
(d)	14 the fresh	3	0	3				14 chain	6	1	7		
	15 air	15	36	51				15 of demonstration	4	2	6		
	16 encompassed her	13	2	15				16 are to the beginner	26	4	30		
	17 played with	42	3	45				17 things absolutely new	14	4	18		
	18 the downy	34	7	41			(n)	18 they must be fixed	7	3	10		
	19 hair	65	7	72				19 by the plastic	15	1	16		
	20 of her neck	39	5	44				20 power	0	3	3		

	FLAUBERT.							BAIN.					
	I	II Words and Ideas.	III Ideas Only.	IV Total.	V Group Totals.	VI %		VII	VIII Words and Ideas.	IX Ideas Only.	X Total.	XI Group Totals.	XII %
	21 blew about	23	14	37	(d)			21 of contiguity	20	4	24	(n)	
	22 her side	3	1	4				22 and time	5	0	5	76	9.5
	23 the strings	27	2	29	414	27.6		23 and concentration	13	1	14		
	24 of her apron	45	1	46				24 must be allowed	2	1	3		
	25 which twisted	6	1	7				25 for the purpose	0	1	1		
	26 and twirled	4	13	17			(o)	26 but in a good head	9	5	14		
	27 like a flag	22	1	23				27 one or two	5	6	11		
	28 little	16	1	17				28 examples	4	0	4	(o)	
(e)	29 once a thaw	27	8	35				29 of each	1	0	1		
	30 had set in	10	5	15				30 strongly	2	4	6	67	8.4
	31 the water	23	5	28				31 imprinted	5	7	12		
	32 was trickling	31	14	45				32 will make easy	7	7	14		
	33 from the bark	25	4	29	(e)			33 the rest	5	0	5		
	34 of the trees	37	0	37	280	28	(p)	34 the method	0	0	0		
	35 and snow	22	2	24				35 or character	0	0	0	(p)	
	36 was melting	24	1	25				36 of the devices	2	2	4		
	37 from the roofs	30	4	34				37 will be seen through	0	2	2	12	2.4
	38 of the buildings	4	4	8				38 and acquired	0	6	6		
(f)	39 she stood	5	1	6	(f)		(q)	39 and in every case	0	0	0		
	40 on the threshold	6	2	8	14	7		40 new	1	2	3		

	FLAUBERT.							BAIN.					
	I	II	III	IV	V	VI		VII	VIII	IX	X	XI	XII
		Words and Ideas.	Ideas Only.	Total.	Group Totals.	%			Words and Ideas.	Ideas Only.	Total.	Group Totals.	%
	42 to get	33	14	47				42 will fall back	1	7	8	(q)	
	43 her parasol	70	4	74				43 on the old ones	1	4	5		
(h)	44 she opened it	22	6	28	(h) 28	28		44 for the element	1	0	1	28	4.
(i)	45 the parasol	18	5	23				45 common	2	2	4		
	46 was changeable	54	2	56			(r)	46 and concentrate	0	2	2		
	47 green	60	7	67				47 attention	0	0	0	(r)	
	48 blue	34	0	34				48 solely	1	4	5		
	49 silk	52	1	53				49 upon difference	8	2	10	17	4.25
	50 and the sun	35	9	44			(s)	50 when after going over	0	1	1		
	51 shining	35	5	40	(i)			51 a few	3	1	4		
	52 through it	28	2	30	466	33.2		52 definitions	3	0	3		
	53 lent	12	9	21				53 the learner	0	1	1		
	54 a radiant	8	2	10				54 is impressed	2	1	3	(s)	
	55 and flickering	4	8	12				55 with the form	3	5	8		
	56 lustre	7	25	32				56 and peculiarity	0	1	1	29	2.6
	57 to her white	25	11	36				57 of a definition	1	1	2		
	58 complexion	28	14	42				58 there is little	2	3	5		
(j)	59 she smiled	5	0	5				59 to acquire	0	2	2		
	60 beneath it	4	6	10				60 in the rest	0	0	0		
	61 the soft	3	0	3	(j)		(t)	61 a slight	5	2	7		
	62 zephyrs	13	6	19	52	8.6		62 substitution	8	5	13		

FLAUBERT. BAIN.

	I	II Words and Ideas.	III Ideas Only.	IV Total.	V Group Totals.	VI %		VII	VIII Words and Ideas.	IX Ideas Only.	X Total.	XI Group Totals.	XII %
(k)	63 played	8	2	10				63 serves to make	2	2	4	(t)	
	64 about her	5	0	5				64 a new one	6	3	9	39	7.8
	65 and raindrops	40	5	45				65 out of an old	5	1	6		
	66 were heard	11	0	11			(u)	66 the definition	49	0	49		
	67 to come down	6	17	23	(k)			67 of a square	52	0	52		
	68 pattering	26	7	33	166	20.7		68 is changed	37	13	50	(u)	
	69 one by one	5	3	8				69 easily	40	9	49		
	70 on the outstretched	5	5	10				70 to suit	16	13	29	290	48.3
	71 silk	13	1	14				71 a rectangle	59	2	61		
	72 of the parasol	18	4	22									
	Totals				2458	34.1						1232	17.5

her parasol of changeable green and blue silk and the sun shone through it."

In columns VII.–XII. are given the same statistics for the extract from Bain as were given for that from Flaubert. It will be seen that only about half as much of Bain was remembered as of Flaubert. In this all that was remembered by more than 25% of the men was that "The subject matter of geometry embodies a few fundamental notions and processes; a definition, an axiom, a postulate, a proposition, whether theorem or problem, are to the beginner things absolutely new. The definition of a square may be easily changed to suit a rectangle." The rest of the extract will be seen to have impressed only from 2.6–9.5% of itself upon the hearers. I think the total and total per cent. for this passage would have been less, too, had it not terminated with the very definite statement concerning quite familiar concepts which it did.

As to the mental imagery involved, in the memory of the foregoing passages, it is necessary to note the following: Aside from the evident adaptability of the extract from Flaubert to an easy visual resuscitation, and the obvious difficulty in representing that from Bain in any sense imagery, except the auditory imagery of the words themselves, both of which characteristics are fully shown in the statistical results of the experiment, it will be seen that the visual mental imagery is more commonly the form in which the reproduction takes place, and that where the visual is not awakened the auditory comes in to help.[1] The auditory resuscitation includes, of course, both sounds and words. There was in the Flaubert only one purely auditory image called up besides those of the words themselves, viz.: "The rain-drops were heard to come pattering down, one by one, on the outstretched silk of the parasol."

In group *n* (Bain) the only words which were above the average were *plastic*, *contiguity* and *concentration*, and the first two of these, at least, were uncommon words, and have a peculiarly scientific and philosophical sound. It was certainly the sound rather than the sense of the word which caused one man to write '*continuity*' instead of '*contiguity*.' In sev cases these words were put down without being made in sentence, which seems also to corroborate the supposition the words were remembered as auditory or motor images.

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[1] Corroborating the results of Jonas Cohn, Bigham, Müller and Schumann, etc.

Experiment B.

My most extensive research was among 125 well-kn painters and sculptors of New York City, to find what part conscious visualization played in their work. I found not that there is no very extraordinary power among these art to visualize things more vividly than among ordinary peo but that some painters seem to have the imagery developed little. Almost the only notable exception is a very well-kn artist who paints pictures of rural life with almost photogra fidelity to detail. He told me that he studies and consta employs his visual mental imagery in painting, constantly re ring to it in the manner of Wm. Blake, the English visiona

I sent these artists a 'Mental Imagery blank,' contai eight questions, the first four taken from Galton,[1] the last being Professor Cattell's.

The results of this research as answers to the blank ar be summarized as follows:

To question 1. Think of some definite object, suppose your breakfast table as you sat down to it this morning, consider carefully the picture that rises before your mind's

Are the outlines of the objects distinct and sharp?

Yes.	50%
Fairly.	11%
Yes. (Of things drawn.)	1%
Distinct, not sharp.	4%
No.	33%

To question 2. *Are the colors bright and natural?*

Exactly as in nature.	1%
Yes.	80%
No.	11%
Subdued.	7%

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[1] *Inquiries into Human Faculty*, p. 378.

To question 3. *Where does the image seem to be situated?*

Behind the head.	1%
Impossible to say.	2%
In the head.	20%
Before the eyes.	40%
At a distance.	22%
Exact original distance.	16%

To question 4. *How does the size of the image compare with the actual size of the scene?*

Same.	73%
Smaller.	21%
Larger.	3%

To question 5. *Can you call to mind better the face or the voice of a friend?*

Face.	70%
Face, then voice.	4%
Both at once.	5%
Either.	12%
Voice.	8%
Neither.	1%

To question 6. *When violin is suggested, do you first think of the appearance of the instrument or the sounds made when it is played.*

Appearance.	80%
Sound.	20%

To question 7. (*a*) *Can you call to mind natural scenery so that it gives you pleasure?* (*b*) *music?* (*c*) *fruit?*

Scenery.	96%
Music.	92%
Fruit.	68%

To question 8. *Have you ever mistaken an hallucination for a perception?*

Visual.	12%
Auditory.	14%
Tactile.	1%

I received many letters from the artists besides the answers to the blanks. These contained much light upon the latter, in most cases corroborating, but in one or two cases contradicting (unconsciously) the statements in the answers to the printed questions.

Most interesting are several in which the writers describe the way in which ideas for pictures come to them. Says one:

"My impressions have at times been so strong and the location so immediately behind my head that the impulse has been great to turn around to see the scene or figure. When an inspiration or design for a composition comes to me, though my head may be bent forward upon my hands, the image is back of the head or more properly above it and often so distinct that I have drawn out the entire plan of composition with eyes closed."

Says another: "I myself seldom paint from memory, but when occasion occurs I find I can quite readily carry in my mind for several days and in some cases for years a very true impression of a scene. Some artists do this altogether and never employ models. Of course, to do this one must study the scene with the object in view of carrying it in the memory. Take for instance a subject that I studied three years ago with the intention of painting it. I was crossing the river in a ferry boat and, sitting here in my room now as I recall the subject, the room about me seems to vanish and I see before me the river at twilight, dark and full of movement, the sky just correct in value compared with the river and with sundry yellow streaks at the horizon. In the immediate foreground a slender figure leaning somewhat over the rail of the boat. The objects in the boat are all in their place—anchor, bucket, etc., and there are several red and white lights on the Jersey shore."

Says another: "I not only remember the table with the people, dishes and general detail, but the background as well. For years I have practiced the idea of Robert Houdin and his brother of taking [in] or studying an object in as short a time as possible with the eye, then turning away and either drawing or describing it. I remember once making a small oil picture of a city house for a lady who constantly sat at her window and who would have noticed me if she had seen me stop to even make a note. * * By walking slowly by on the opposite side, not more than three different times I made a carefully detailed picture, it being a corner house with some odd buildings in the background made it a little more difficult of course to recall, but each morning as I passed I looked the object carefully over, repeating or speaking to myself the detail from cornice to stoop and so on."

Another writes: "I spent two months at the World's Fair making careful and elaborate pictures of the buildings and various views. This winter I have been enlarging those pictures in my studio and have frequently noticed myself in that state of feeling as of being actually back at Chicago and in the original spots where the various pictures were made—at times the same airs of the bands and cries of the vendors of catalogues, etc., recurring to me."

A sculptor writes: "From my experience in thinking out ideas to illustrate certain given subjects I have frequently seen the imaginative subject so realistically developed as to startle and shock my nervous system. I can only think out subjects, however, to perfection in the dark, preferably in bed—the subjects generally appearing some distance away when clearly developed."

A painter well known for his mural decorations: "Personally, I would say that this image as conceived is rather hazy * * * and while there is the sense of seeing it, I seem to feel it quite as much because the personality of a figure so conceived as existing has to me characteristics as well as clothes. If it is an idea involving many figures it is always limited to a very narrow field in which the central or keynote of the composition is more or less suggested. It is on this that work is begun, resting content to allow the actual result as attained on canvass or paper [to?] stimulate the mind to further developments. It may be on this account, perhaps, that the first third of the picture under way moves along rapidly and with few hitches, while the second becomes involved with labor and annoyance and the last third is almost constant irritation and disgust."

Another writes: "As he (the true artist) paints in his studio, without once referring to an out-of-door sketch, far better than the imitator, he must in some way retain a very perfect impression. I can assure you, however, he does not keep one eye on a mental picture while painting. He looks and sees only the canvass, and that, as he will tell you, 'feels' right or wrong."

And another: "In calling up a mental picture of a scene I seem to see objects at the same distance, of the same size and in the same light as they were in reality. They seem to surround me as they did in fact. The image is not definite. Even when it seems to my mental vision tolerably complete the instant an attempt is made to draw from it I find it utterly vague with great unfilled gaps. To reproduce a view once seen is much more an effort of memory than of calling up a mental image. For instance, if I remembered that there were flowers on the breakfast table, I should at once place them in my mental image, but I should hardly see them in the mental image and so remember that they were on the table. Even if my recollection were incorrect and there were no flowers, I should probably call them up and add them to my mental picture if I thought they were there."

And another: "In composing or imagining pictures [I] can call to mind objects more readily with my eyes closed. Even getting at the drawing, foreshortening, etc., with tolerable exactness. For instance, sitting with the eyes closed and thinking very hard of the subject I wish to paint, the picture appears as framed and hung in a gallery. Thus I can make a sketch almost as correctly as though I were doing it from memory after actually seeing a picture.

A pertinent comment upon the question concerning the violin is found in the following words of a very well-known genre and mural painter: "I answer 'the appearance.' It would be more accurate to say the form and the form as seen in certain positions which I have studied for the decorative beauty of the line. But of many other instruments I should say the contrary. 'Trumpet,' for instance, expresses sound to me and so do 'horn' and 'flute.' A trombone is so grotesque in its method of operation that that fact occurs to me first. A harp is a decorative form. Of course a lyre is a mere form to all of us. We have never heard it. I think these instances show that association plays a great part in determining our way of imagining things. Also the word *violin* would have an entirely different effect on me if used in a sentence dealing with a piece of music. Then I should think of its nasal singing."

In addition to these answers and the letters of explanation sent me at the same time I have a great deal of material bearing upon the mental imagery of artists in the shape of reports of personal interviews. I called upon about twenty-five artists in their studios and put questions to them orally, in this way endeavoring to corroborate the results given by the blanks. I carried a little notebook with me, in which were the following questions, for which, where they are not Galton's questions, I am largely indebted to Professor Cattell:

"1. Does your train of ideas when it proceeds without external stimulation go on in terms of sights, sounds, etc., or words? If words, are they heard words, impulses to articulate or what?

"2. What class of sensations do you most easily recall?

"3. Tell me something more at length about the (*a*) illumination; (*b*) definition and (*c*) coloring of your mental imagery.

"4. Does your mental imagery seem to embrace a greater extent of view than the external visual field?

"5. At what distance do the mental images seem to be?

"6. Have you good command over your mental imagery; *e.g.*, can you make your mental image of a person appear to sit down or walk about?

"7. Do you recall the appearance of persons with whom you are familiar better than that of others?

"8. When reading a book do you mentally illustrate descriptions of natural scenery, etc.

"9. Have you any peculiar preferences for, or associations with, colors?

"10. Is your mental imagery fainter now than in childhood?

"11 In terms of what sense is your present recollection of a play or an opera?

"12. Does poetry appeal to you as pictorial?

"13. Do you paint from memory?

"14. Do you recall the sounds of nature?

"15. Do you recall music?

"16. Do you think of the name of an object when it is presented to you? of a person when you meet him?

"17. When you read to yourself do you seem to hear any one's voice saying the words? Your own voice or the author's if you happen to know it? When you come to printed italics does this voice seem more intense?

"18. Do you rely upon mental words when doing sums in arithmetic? In writing do you spell out every word, seeming to see or hear the letters individually as you write them?

"19. Do you think in any language other than English? If so, in heard words, impulses to articulate or what?

"20. Do you think the expression 'hear yourself think' would describe your mental state at any time? Do loud noises interfere with your work?

"22. Can you think the word 'bubble' with open mouth. (Stricker and James.)

"23. When *Apple* is mentioned do you think of the taste or the appearance?

"24. Have you mental imagery of the following: Tastes: salt, sugar, lemon, raisin. Smells: tar, roses, lamp, hay, violets. Touches: velvet, silk, soap, sand? Heat, cold, hunger, thirst?

"25. Do you become seasick? Could you make yourself seasick by imagining the rolling of a steamer, etc.? Do you become dizzy when at a great height? Can you make yourself dizzy by imagining yourself on a frightful precipice?

"26. Can you imagine a violent movement without the impulse to make it? *e. g.*, mentally fire off a pistol without in reality catching your breath, or mentally ride horseback without in reality pressing your knees together just a little?

"27. Does the emotional background of your consciousness vary from day to day?

As a rule I called upon only one artist a day and took the first opportunity after leaving his studio to record in my notebook exactly what had been said.

Statistical results from the oral questioning just described are almost impossible to collate. I have made, however, an extensive table of the results, which I will attempt partially to summarize.

That these questions were in one or two cases almost tautological with others will not, I think, detract from the value of the answers, but quite the contrary. I should have been better satisfied if I had had the time to put the questions in as many forms as I could think of, to every artist. I felt somewhat diffident, however, about questioning them with too great prolixity. I take pleasure here in thanking them for their uniform courtesy in giving me so much of their time and attention.

Out of 25 artists orally examined by me, 20 said their train of thought went on in scenes, pictures, etc., and 20 said that the sensations most easily and vividly recalled were those of sight. One of these, however, a sculptor, put the vividness of his sense representations in this order: Touch, organic sensations, sounds and sights.

Fourteen of the twenty-five said they had no greater field of mental than of physical vision, and three stated that they had a greater. One of these was the painter who located the images at the back of his head at a distance of three feet.

Thirteen of the artists said that they thought their command over their visual mental images is good. Two said their images do not move. One said that his images of people do not move because for years his only training (as that of all artists) has been to 'see them still.'

Nine said they recall familiar faces better and two said no better.

Those who recalled natural scenery in mental imagery were seventeen. (Nine not reported.)

Twelve of these mentally illustrate books as they read, and four say they do not mentally illustrate.

As would naturally be expected, color preferences are exceedingly diverse, and peculiar associations with colors are frequent. The order of preference for the twenty-seven artists under consideration now is, red, yellow, blue, brown and purple, violet and gray, the last being least preferred.

Among the peculiar associations is that of dizziness with red. One artist said that he became dizzy if his whole field of vision was filled with red, mentioning a matador's cloak which hung in his studio.

Two have a curious attitude towards blue. One likes it, but finds it hard to manage. Another, who prefers reds, said of blue that he feels towards it as if it were on a different (higher) social plane than his own.

Another described his attitude towards certain colors thus: Yellow is salt to him, accountable possibly on the score of salt butter. Green is acid, purplish greens being worse; they are bitter, acrid. Red is peppery. Blue stands for shadow and emptiness. Yellow is his favorite color, but that is indefinite, for his liking for colors depends entirely upon the *nuance*.

Of the eight only who gave definite answers to the question (10) "Is your mental imagery now fainter than in childhood?" five said it was and three said it was not fainter but better. As proof of this, one or two cited examples of quasi hallucinations seen in childhood, imageries which were much more vivid than theirs of the present day, but the others, generally, said they had noticed a gradual fading of their mental imagery.

One of those who said his mental imagery is better now than ever, paints largely from memory and visualizes numbers and the chess board.

Of the eight who replied to the question "Do you paint from memory?" only one gave a negative answer.

Of the seventeen who answered the question, "Do you seem to hear any voice in your mind's ear when reading to yourself?" seven replied in the affirmative and ten in the negative. Of the former one, a sculptor, seems to hear his own voice *raised* when he reads italics.[1]

Of the foregoing seventeen, five dispense with mental words in doing sums in arithmetic, eight do not and four are doubtful.

Of the seventeen whose thinking in words was questioned eight seemed to articulate and five seemed to hear the words. One seems to hear the words only. The others found it impossible to analyze.

Eight could think the word bubble when the mouth is open and two could not. One of the latter seemed to articulate when he read to himself and neither seemed to hear when reading to himself.

The appearance of the apple was brought before the minds of all who answered the question concerning the apple's taste or appearance.

In recalling sensations of touch, taste and smell, all who were questioned, except five, lacked the memory of at least one of these, some two.

Among the twenty-seven artists orally questioned I found only two with number forms. One other had a form for the week.

Experiment C.

Another method of studying the mental imagery of other people is the examination of works of the imagination. All works of the imagination must give scope for the mental imagery. In poetry in particular we see a wide field for the study of imagery of various kinds. We can infer from the carefully polished lines of Tennyson that he had a good auditory mental imagery and also from the references to sound which occur in the poems, we may see that little that is beautiful in pure sound escaped him, and that the auditory mental-imagery was a potent factor in the creation of his poems.

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[1] One, who thinks he mentally articulates, says that, after reading to himself a few pages of Browning, his jaws become tired.

And it may be seen also in other poets that various sensuous elements were dominant in their minds. Swinburne seems to revel in the sound of the wind, and his verses are full of the 'windy suspirations of forced breath' in the letters of the alphabet most numerous in the words he uses. In *Atalanta in Calydon* sibilants and fricatives are used more than any other class of consonant. If he can, he uses a word with f, v, s, or sh in it, as if to give it an atmosphere of blowing winds.

Browning strengthens and roughens the sound of his verse by the use of a great number of gutturals and dentals.

CONSONANTS IN BROWNING, SHELLEY, TENNYSON, ETC.

I counted the consonants in eight pieces of literature, five of which were parts of poems and one a part of a play (Othello), the other two being a passage from Burke and a passage from Green. I used the consonant scheme found in Sweet's *Handbook of Phonetics*. The results are as follows, arranged first in groups of similar consonants:

TABLE II.

AUTHOR.	CONSONANTS.							
	Liquid L, M, N, R, NG	Mutes.			Sibilants SH, S, Z, D, DJ	Fricatives V, F, *TH*, TH	Semi-Vowels W, WH, J, H	No.
		P, B	T, D	K, G				
Green,	34.35	6.00	15.95	6.05	14.65	14.75	6.80	2000
Burke,	37.32	6.56	18.34	5.20	15.92	13.12	5.46	5000
Tennyson,	38.60	5.10	17.80	4.45	11.87	13.70	7.60	2000
Browning,	33.10	6.85	16.50	5.85	17.05	12.05	7.25	2000
Swinburne,	37.25	4.10	16.90	3.10	14.15	16.90	6.85	2000
Shakespeare, (Venus and Adonis),	34.05	6.35	17.80	5.45	15.60	11.25	8.80	2000
Shakespeare (Othello),	34.22	5.74	15.28	4.94	15.30	12.34	8.98	5000
Shelley,	37.06	5.10	17.68	4.52	14.10	12.30	7.84	5000
								25000

Table III.

Per Cent. of Each Consonant in the Eight Examples.

Letters.	Burke.[1]	Tennyson.[2]	Swinburne.[3]	Venus and Adonis.[4]	Othello.[5]	Shelley.[6]	Green.[7]	Browning.[8]
SH	2. +	.9+	1.3+	2. +	1.	1.	1. +	1.
P	3. +	2. +	1.6+	2. +	2. +	3.	2. +	4.
W	1.8+	2. +	3. +	2. +	2.	3.	2.	2.
H	1.6+	3.7+	2. +	4. +	3. +	3.	3.	2.+
F	3.	3.	5. +	3.1	3.	3.	3.	3.
M	4. +	5.	3.	3. +	5. +	5.	3. +	4.
D	8.	7. +	8.	7. +	6. +	8. +	7. +	6.
S	6.9	6.12	6.	7. +	8.	7. +	7. +	10.
T	10. +	10.	8.	9. +	8. +	9.	8.	10.
R	12. +	12. +	11.	11.	11. +	11. +	11. +	9.+
N	12. +	11. +	12. +	10.	10. +	11.	11. +	10.
L	6.	8.	7. +	6.	5.	7.	6.	7.+
TH[9]	5.7+	6. +	7. +	4.	5.	5.	5. +	5.
Z	5.7+	4. +	6.	5.	5.	5.	3. +	4.
K	3.9	3. +	2.	3. +	3. +	3. +	4.	4.
B	3. +	2. +	2.	3. +	2. +	2.	3.	2.+
V	3.8	2. +	3. +	2. +	3.	2. +	4.	3.
NG[10]	1.7+	1. +	2. +	2.	.9	1. +	1.	1.
G	1.2+	1. +	1.	1. +	1.	1.	1. +	1.
J[11]	.9+	.8	.4	.25	2.	.7	.8+	1.
TH[12]	.5	1.2	.45	1.	.8+	1.	1. +	1.
TS	1.3+	.8	.4	1.	1.	.8	.9	1.+
DJ[13]	.8+	.7	.3	.4	.8	.3+	1.	1.
WH	1. +	.5	.15	.7 +	1.	.9	.5	1.
ZH[14]	.1	.05	0.	0.	.06	.06	.25	.1

[1] The 5,000 consonants from Burke's *Revolution in France*, Vol. 2, pp. 87-94 in the Clarendon Press edition of his works.

[2] The 2,000 consonants in the first 117 lines of Tennyson's *Enid*.

[3] The 2,000 consonants in the first 111 lines of Swinburne's *Atalanta in Calydon*.

[4] The 2,000 consonants in the first 115 lines of Shakespeare's *Venus and Adonis*.

[5] The 5,000 consonants from the opening of *Othello*.

[6] The 5,000 consonants in the first 186 lines of Shelley's *Adonais*.

[7] From Green's *Short History of the English People*.

[8] The 2,000 consonants of the first 117 lines of Browning's *The Ring and the Book*.

[9] TH = th in 'than.'

[10] NG = ng in 'sing.'

[11] J = y in 'yes.'

[12] TH = th in 'thing.'

[13] DJ = j in 'just.'

[14] ZH = s in 'pleasure.'

In the preceding tables it will be noticed that Swinburne's use of the fricatives in *Atalanta and Calydon* is very great, being nearly five per cent. greater than Browning's, and that Browning has a large percentage of sibilants. The liquids are most numerous in Tennyson, Burke, Swinburne and Shelley, and least numerous in Browning.

The two excerpts from Shakespeare show a general likeness that is remarkable, there being a great difference only in the mutes T and D. His liquids are remarkably few and the other consonants do not differ much from the average except in the case of the sibilants and semi-vowels, which are greatly in excess of those of the other poets examined. It is hardly fair, however, to judge Shakespeare by modern pronunciation.

Tennyson, shows besides a preponderance of liquids, a large number of lingual mutes, the others being below the average, the smallest number recorded of sibilants, about the average of fricatives and a little more than the average of semi-vowels.

Browning has, compared with the average, few liquids, few lingual mutes, many labial and palatal mutes, very many sibilants, few fricatives and few semi-vowels.

Swinburne has many liquids, very few mutes, few sibilants, very many fricatives and few semi-vowels. (Burke, alone, has fewer semi-vowels.)

Shelley has very many liquids, few mutes, except linguals, few sibilants, few fricatives and many semi-vowels.

The table on the following page shows the relative frequency of each consonant.

It must be that the poet whose rough consonants are greatly in excess of the average has either a less sensitive 'ear' than the one whose liquids are the most numerous, or uses them as an expression of strength. Now, an 'ear' in the sense above used means the possession of mental imagery of the auditory kind and we may then say that Tennyson and Shelley had a better ear than Browning.[1] That Shelley's liquid N is so far in

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[1] 'Ear' does not mean here, however, a capacity for understanding and appreciating good music, for it is well known that Tennyson and Swinburne have no ear in this sense and that, curiously enough, Browning had. He was, as an amateur, a thorough musician.

TABLE IV.

Tennyson.	212 R	200 N	172 T	137 L	130 D	117 TH	107 S	90 M	74 Z	68 H	57 K	56 F	46 B	45 V	42 W	38 P	29 NG	28 G	20 TH	17 SH	14 J	13 DJ	12 TSH	8.2 ZH, WH
Browning.	179 N	175 S	173 R	170 T	126 L	104 D	85 TH	79 K	75 Z	69 M	66 P	55 F	50/49 H, V	44 B	39 W	26 TSH	24 G	23 NG	22 DJ	21/20 SH, J	19 WH	16 TH	2 ZH	
Venus and Adonis.	194 R	179 N	165 T	132 S	131 D	106 L	90 Z	80 H	74 TH	69 K	64 B	63 M	54 W	53 F	49 SH, V	47 P	39 NG	27 G	24 TSH	19 TH	14 WH	6 DJ	2 J	
Othello (2d series).	190 T	189 N	183 R	123 D	120 S	90 M	87 TH	86 L	85 Z	66 H	64 K	59 F	54 V	50 B	46 P	40 W	39 J	20 SH	19 G	18 NG	16 TH	14 TSH	13 DJ	11 WH
I. Swinburne.	236 N	199 R	159 D	150 T	136 L	135 TH	114 Z	112 S	97 F	70 V	68 W	59 M	50 H	44 B	43 NG	36 K	31 P	24 SH	20 G	9 TH	7 TSH	6 DJ	4 J	2 WH
II. Swinburne.	215 N	209 R	174 T	159 D	135 S	125 L	101 TH	92 Z	82 M	80 F	62 W	61 P	51/50 K, B	49 V	43 H	41 NG	30 WH	25 SH	24 G	23 TH	14 TSH	6 DJ	1 ZH	
I. Shelley.	211 R	186 N	160 T	141 D	131 L	114 S	92 TH	90 Z	86 M	67 K	64 P	62 H	60 W	58 F	42 V	41 B	26 NG	23 WH	21 P	17 TSH	16 SH	14 G	11 J	5 DJ
II. Shelley.	236 N	216 R	165 D	156 T	146 S	129 L	114 M	112 Z	100 TH	64 W	61 K	51/50 H, F	49 P	40 B	39 V	33 NG	30 TH	24 SH	23 G	14 TSH	12 WH	11 J	9 DJ	3 ZH
I. Burke.	205 T	203 N	169 R	152 S	119 L	115 D	89 TH	76 V	69 M	65 Z	64 F	61 K	49 B	48 H	39 SH	36 P	29 NG	27 G	23 TSH	20 W	18 DJ	15/14 J, WH	9 TH	4 ZH
II. Burke.	194 N	169 T	155 R	131 S	107 D	84 Z	83 L	72 M	69 K	64 TH	63 V	54 B	53 P	44 F	36 SH	29 W	24 NG	19 G	16 WH	15 H	11 J	6 TH	1 ZH	

advance of all the others except the same letter in Swinburne argues a conscious preference for that letter. And this is not affected by the fact that in all cases except Othello the letters R or N heads the list.

Tennyson (with several others, such as Rossetti) seems to have paid particular attention to the mental imagery of the five senses, that his poems would excite. His appeals to the visual imagery are often pictures with a studied effect *qua* pictures,[1] and while we listen to his voice we see more than we hear. He is most careful to change the pictures not too rapidly and to present them in such an order that they shall have the best effect upon the mind's eye. There is, occasionally, in one of his lines nothing more than a single visual, pictorial aspect of what he describes, *e. g.*, from the *Princess:*

> "And I sat down and wrote,
> As when a field of corn bows all its ears,
> Before the roaring east."

Such lines of Tennyson and some of Wordsworth and of Rossetti and others having nothing but a purely visual content, are to be contrasted with many lines of other poets, which contain only the sound of the words and possibly a slight emotional or organic imagery. If we attempt to visualize such lines as

> "To take up arms against a sea of troubles
> And by opposing, end them."

the result is often the purest visual nonsense. These lines have indeed made great trouble for the commentators.

Experiment D.

I have examined, also, the first 1,000 lines of Tennyson's *The Marriage of Geraint*, and the first 1,000 lines of Browning's *The Ring and the Book*, marking in each the various imagery that are awakened by reading the lines. In doing so I can only take myself as an example of the average reader.

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[1] Cf. *Oenone*, lines 90–100 (the poem contains appeals to all the five senses).

Here are the results:

In the first 1,000 lines of There are	Tennyson. Examples.	Browning. Examples.
Visual imagery, ...	83	107
Auditory imagery, ...	48	40
Olfactory imagery, ...	0	2
Motor imagery, ...	1	10
Thermal imagery, ...	1	3
Tactile imagery, ...	7	11
Organic imagery, ...	3	4
Gustatory imagery, ...	0	4

Examples of the various imageries as they appealed to my mental sensations are:

VISUAL.

Tennyson.	Browning.
But never light and shade *Coursed one another more on open ground.* *Beneath a troubled heaven than red and pale* *Across the face of Enid.* l. 521.	I saw the star supposed, but fog o' the fen, *Gilded star fashion by a glint from hell.* l. 544

AUDITORY.

Each of whom had broken on him A lance that *splintered like an icicle.* l. 938.	Once let smoke rise untroubled, we decry Clearlier what *tongues of flame may spire and spit* To eye and *ear.* l. 935.

OLFACTORY.

	Who, breathing musk from lace work and brocade. l. 919.

MOTOR.

At this he *hurled his huge limbs out of bed,* And *shook* his drowsy squire awake. l. 124.	Do you see this square old yellow book I *toss* *I' the air and catch again and twirl about?* l. 33.

THERMAL.

Made her *cheek burn* and either eyelid fall. l. 775.	[I] stepped out on the narrow terrace built Over the street and opposite the church, And paced its lozenge brick work *sprinkled cool.* l. 478.

TACTILE.

Whom first she kissed on either cheek, and then
On either shining shoulder *laid a hand.*
l. 517.

And since the course was much to his own mind
Of pinching flesh and pulling bone from bone,
To unhusk truth a-hiding in its hulls.
l. 977.

ORGANIC.

She found no rest and ever failed to draw
The quiet night into her blood.
l. 531.

The not unpleasant flutter at the breast.
l. 860.

GUSTATORY.

Taste some vituperation, bite away.
Whether at marjoram sprig or garlic clove.
l. 313.

The auditory imageries are, however, made up largely of 'conversation,' for which I seem to fancy the voice of the speaker.

All these are, however, but objective methods wherein the result is accurate only to the third or fourth remove, because of misunderstanding of questions, of wilful perversion of answers and of the great difficulty of collation and comparison, as well as of the danger of reading my own mental imagery into the words of the others.

The only way, then, in which the phenomena of mental imagery can be studied at first hand and brought to a quantitative result is by my examination of my own mental imagery.

I began to study my own mental imagery in 1894, while reading a work on philosophy, which, I soon observed, did not wholly take up my attention. I was not in very good physical health and my mind wandered extensively. I soon appreciated that there was more interest in watching the inconsequences of my mind, which were exhibited in a rapid sequence of sense images, than in trying to 'make sense out of' the abstract language of the philosophy book. So I made a record of the times when my mind wandering or, as I should call it, my secondary thought-current entirely superseded, to its complete obliteration, that aroused by the words of the book. This secondary thought-current may have resulted from my vain attempts to read sense imagery into the abstract language of the book I had been reading—a sort of reaction.

My attention having been fixed upon the secondary current (secondary only in being apart from my appointed work; it was primary in intensity) I proceeded to make records of it. I found that even though I approached the sense imagery consciously, it seemed to pass as regularly as, and more freely than, before. I tried it one or two nights before going to bed, and in the afternoons after some hours of philosophical reading.

After several experiments I devised the following

METHOD OF OBSERVING AND RECORDING MY OWN MENTAL IMAGERY.

Experiment E.

I take a piece of paper with fifty spaces marked on it, and sitting at my desk in as quiet a time as possible, I write down in each space a word or words descriptive of the mental imagery which goes on before my mental eye, ear, etc., hurrying from one word to another until the fifty spaces have been filled. Then I take the time elapsed and as quickly and as carefully as may be I look over the words and record whether what they stand for is visual or any other mental imagery. This, after some practice, is comparatively easy.

I appreciate the crudeness of this method, but I was unable to devise a better, for words seem to be the only way of recording mental imagery.

When performing this experiment the mental imageries change with the greatest rapidity and almost maniacal incoherence, and I confess that only a portion of the mental images are transferred into words. Some of them cannot be on account of the poverty of the language and others are not to be expressed in single words or short phrases. On the other hand, to write out afterwards a complete description of the mental imagery which I had already recorded during one five minutes, would be impossible, for, in the time taken to describe that current, new mental imageries would arise which did not really belong to it, and it would be impossible exactly to discriminate between what mental imagery occurred from 11.00–11.05 a. m., for instance, and that recurring in the remainder of a half hour.

The words, of course, dragged in a good many pictures, sounds, smells, etc., which would not have occurred to me if I had not tried to record my train of thought, nevertheless the result was a number of mental images of various kinds which, I believe, showed the tendency, at any rate the possibilities of my mind at that particular time. And, after recording many sights, a few sounds, etc., even if I did think to myself, "Suppose you have a few smells, tastes and touches occur to you," I believe, even so, that shows, as well as anything else, the quality of the mental imagery that I may have.

It seems again to be a matter of total indifference whether the lists be made out in entire isolation or not. If I do see or hear distracting things while writing the list, all that is necessary is to put down words descriptive not of the sensation, but of the mental images. If the 'thought stream' be all sensation and no mental images, then wait until mental imagery begins again and observe that alone.

Here is a sample of the blanks as I fill them out myself:

12/30/97//11.08 A M 4¼ min. 5 balks[1]
V 3 7 (visual) OR 1 (organic) M 1 (motor)
W 9 (words only) OL 1 (olfactory) A 1 (auditory).

V F——
V bookstore
W Lester
W Kester
A N—
V Mr. M——
V El. Road
V Mazetti's
V Duquenne's
V tables
O smell
V C——
V M——
V castle
V A—— B——
V her home
V her garden
W self reading paper
V psy. lecture room
V C——
W words
V Mc. W——

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[1] By *balks* I mean the absolute blank which is the state of my mind when neither words nor images come. This will be noticed below.

W of D's restaurant
V steam
W visual
V deliv. wagon
V real est. ag.
V W——
V 6th ave
V palmist's sign
W Paderewski
M holds self to seat
V Mac D——
V college buildings
V clean
V fresh
V exercises
V his desk
V window
V seminar
V room
V blackboard
V Mr. T——
W cats and kittens
V pictures of boxes
V woman student
ORG gone feeling
W bad taste
V ground glass
V wood work

Here follow the results of 2,500 recorded mental images collated during two years, 1896–8:

Mental Images.	Per Cent.
Visual.	57.4
Auditory.[1]	28.76
Olfactory.	5.88
Gustatory.	.58
Temperature.	2.0
Touch.	3.84
Organic.	1.1
Motor.	.32
Emotional.	.12
	100.

Average time for each group of 50 experiments, minutes, 7.39
Average number of balks for each group of 50 experiments, 1.46

In some cases my mental imagery has run largely in terms of a single sense. It must be confessed that in such cases, where the thought stream is made up almost entirely of sensations (like smells) usually in the minority, there is generally a synchronous visual current. For instance, in one of the experiments where the imagery reported was largely olfactory, while I had the salt smell of the ocean in my 'mind's nose,' as it were, I quite distinctly visualized a bit of ocean seen from the deck of a steamer out of sight of land, and had a feeling of north of

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[1] Including words without other imagery. Words accompanied by other imagery are ignored for the sake of the imagery.

Cape Cod and east of Gloucester and of looking in a northeasterly direction.

While mentally smelling '*dead fish*' I seemed also to see two figures standing on the shore of Great South Bay looking at something in the sand. (It was a dead balloon fish but at 8.39 P. M. several months later, I did not see that fish in my mental imagery.) There existed then only the smell and the vision just described. I seemed to be at a distance of about 200 feet from the figures in a lane leading to the beach. '*Drift wood*' I seemed to see also—a piece half hidden by the sand (but this was in Connecticut) and worn smooth by the water and sand in which it had been rolling.

'*Wet sail*'—here the scene shifts again to the Great South Bay and I seem to be unfurling or furling a sail on a cat-boat headed S. W. towards Fire Island inlet. Here some of the thought content is visual and some a feeling of direction.

Lavender—this was accompanied by a visual mental image of glass smelling-bottle on a dressing table.

Em.—this is the 'feeling of absence' with the smell not increased, but, I feel quite certain, a dim hazy lavender color before my mind's eye. It may have got the word from the color. I am not sure.

With *Brass* I recall no image save the smell of dirty brass.

Same of *Kerosene.*

Association seems to have worked so fast by words that I got '*tobacco*' without any smell. (I hadn't smoked for nearly a month.)

'*Steel*'—here I *felt* the *touch* also of the back of my razor against my noze, both *smooth* (Touch) and *cold* (Temperature).

With '*old books*' I *saw* the dusty-speckled edge of a paper-covered edition of some novel.

With '*sugar candy*' I had an odor of wintergreen or cinnamon, and the words 'sugar candy' and the appearance of the shiny little satin pillows they call 'buttercups,' which I now appreciate, are not sugar candy at all.

The '*violets*' existed for me more as something cool and dewy touching my nose.

With '*hoof burning*' I seemed to see a cloud of smoke curling out of the open door of a blacksmith's shop where the red-hot shoe was being put on.

With '*aroma*' I seemed to have a dark brown earth color come before my mind's eye.

This is as free from subsequent interpolation as I can make it.

Examples of the Various Types of Mental Imagery Observed in Myself.

I give some examples of each specific manifestation of it as it has come to me in the experiments.

1. *Visual mental imagery* is familiar to every one,[1] and I think all have observed the infinite gradations between the faintest visual imagery and the strongest, the former approaching mere mental blank and the latter almost an hallucination.

A good illustration would be the rising (or the crescent) moon or the falling snow. Any of these is pure visualization. Others are too obvious to require mention.

2. *Auditory Mental Imagery.* I find the auditory mental imagery in my case to be almost as important a factor in my mental life as is the visual, being a mental reproduction of the sounds I have heard—musical or otherwise. They are comparable with real sounds not so much in intensity, but perfectly with timbre, pitch and duration. I can estimate a minute with much greater exactness mentally if I listen to the auditory mental imagery of a piece of music which takes about a minute to perform.

The auditory mental imagery, I would say, includes all the actual word thinking that I do, which is almost always done by means of writing.

See page 42 for analysis of word imagery.

3. *The Tactile Mental Imagery.* This seems to me as clear and strong as any other, occasionally stronger. The imagination of touches gives me real pain, such as the image of touching velvet with the part of the finger which is uncovered

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[1] So visual are we that our very language testifies to it. Among the many instances could be cited that *art* commonly, unless otherwise specified, means pictorial art.

by cutting the nails short. Also it is a really painful thing to think of a finger nail cut or torn to the quick. But the feeling of various things touching all parts of my body, from the feeling of various foods in the mouth (which influences my liking or dislike for certain of them) to the feeling of the water on the skin as one dives into the ocean, there are innumerable things which give in imagination, in mental imagery, almost the same pleasure or pain, as the real sensations gave in the past or will give in the future.

4. *Gustatory Mental Imagery.* In my lists of 2,500 mental images I have registered only twelve instances of the arising of gustatory imagery, and of these only four could be said to be true gustatories, viz.: The tart taste of an apple, the salt taste of sea water, the sweet taste of broiled lobster, the bitter taste of ale.

Sour and bitter are the only tastes I revive quite clearly, the others being three parts visualization and olfactory. I have mental imagery of things that are sweet, such as vanilla wafers, but it is the flavor and the touch (on the tongue) and not the taste which is uppermost.

5. *Olfactory Mental Imagery.* These are in my own case extremely numerous, probably because to me so many things have a smell, often a distinctive smell. One recalls at once in this connection the very potent and sometimes mysterious, associative qualities these sensations have. We are often set to thinking of other times by the recurrence of an odor long unsmelled, and sometimes are at a loss to understand how we should have been thinking of this thing so suddenly. These mental images have to me, like those of the other senses, quite distinctive qualities. The mental image of the smell of new-mown hay is totally unlike, even as a purely mental occurrence, that of the aroma of forest leaf mold. And the words 'tea' and 'coffee' are represented in my mind by two mental images, totally unlike.

The person who traced an association by the subsequently smelled fragrance of a flower had no mental imagery of the smell. I, on the contrary, have had many different olfactory images in a room full of tobacco smoke. The tobacco smoke, however, as one and the same sensation, soon becomes nothing, *qua* odor, and the mind is thus left free to think of others. In quickly changing odors it is harder to imagine smells foreign to the current sensation than to perceive the last.

6. *Thermal Mental Imagery*, or the mental imagery of heat. This is a feeling so slight in intensity that it is hardly perceptible and is, in one instance, a feeling of heat about the head, the feeling of a red face. One can also imagine a hot body approach the face and feel the heat mentally. The percentage of my own thermal mental imagery is only 2% and contains such imageries as 'warm feet,' 'cold nose,' etc. Out of forty-five examples seventeen were 'warm' and twenty-eight were 'cold.'

7. *Motor Mental Imagery*. This is the name which I should give to the mental imagery which has sometimes possession of us when we are imagining ourselves to be running or swimming, or, in fact, moving any part of the body.[1] These, also, form a very small proportion of the mental imageries which I have noted down at the times when I recorded them. These included the imagery of running down a hill and walking. They are so largely contained in visual or other imagery as to be very faint *qua* motor mental images.

8. *Mental Imagery of Pain*. In my regular observation and recording of mental imagery I have not found a single instance of this which, however far removed from the etymological sense of the word imagery, I can yet affirm to exist in me. I can imagine pain, *e. g.*, that of a stubbed toe, a cut, or a pounded finger.

9. *Organic Mental Imagery*. Among these are those of hunger, thirst, etc. These two have not occurred in the lists of mental imageries made during my experiments. But while one cannot "cloy the hungry edge of appetite with bare imagination of a feast," it seems that the imagery of physical plenitude is more likely to occur than the imagery of hunger.

10. *Mental Imagery of Emotions*. This is a kind of mental imagery which, though very difficult to distinguish from the thing of which it is the copy, seems nevertheless to be a verit-

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[1] It probably accounts for the phenomenon of the phantom hand in people who have lost an arm.

able mental state, something quite apart from, and an increment to, a combination of other mental imagery. The thrill which I feel when I think of anything particularly joyous or fortunate may be a mental image or a sensation. Possibly one thrill is a mental image and another is a sensation. Here at any rate is where the former and the latter shade off one into the other. One is here strongly tempted to include with mental imagery all the finer emotions which do not exhibit themselves outwardly by flushed (or pale) face, quaking limbs or trembling voice. But to do this would lead me too far and I shall have to content myself with the description of an emotion of which I subsequently experienced the re-echo or the mental imagery.

On a Staten Island ferry boat going to St. George I sat on the forward saloon deck near one of those small summer orchestras of four or five pieces which are seen on all the Staten Island ferry boats. While they were playing one of the popular airs the steam whistle of the boat sounded a long blast covering ten or twelve bars of the tune they were performing. The experience was pleasureable while it lasted (probably less than half a minute).[1] The first effect of the blast of the whistle was to remove the musicians apparently to a great distance, to rob their music (?) of all but its sawing and scraping and wheezing qualities, which became as soft as the 'voice of a gnat,' and to take all the color out of it. It was as if I had been looking at a colored print and then put a red or purple glass before my eyes through which to look at it. The glass would, by making it all one color, take all the colors out of it and reduce it to a mere outline. And so with the harp and violins in question. The greater noise threw them into distance and made a dangling cobweb out of their butterfly wings. They lost their importance for my attention which was filled with the sound of the steam whistle. I believe they contributed, however, to its effect upon me.

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[1] There is something analogous to this in music, viz., what is known as a pedal or organ point where one note, usually the key note or its fifth, is held through several bars, while chords containing notes discordant to it are played. This gave a far more piquant sensation than any pedal I ever heard. I am unable to say whether, in tone, it happened to be a pedal to the music then being played. I believe not.

Besides the trembling thrill about the diaphragm, caused by the vibrations of the low tone of the blast and by the rumbling of the paddle wheels, the irregular dashing of waves against the bow of the boat—all of which were present in my consciousness at the time—I seemed to feel an abandonment to motion (chiefly visual), a rising above the world, *i. e.*, a feeling of superiority, of regarding things from a lofty and distant point of view, of the transitoriness and bizarre aspect of life; a feeling of reckless irresponsibility, a sense, at the same time, of progress, yet of tranquillity and of eternity—complete leisure and absence of worry in the midst of the most intense motion, swinging, swirling, sweeping, sliding, shooting—rest in motion—the sleep of a spinning top, the sleepiness of the hills on a summer afternoon, and the motion of the earth in space, all in one short moment while the whistle was blowing. When it stopped, I was returned to the world and its motley complex of intuition.

This feeling is produced somewhat by any rhythmical sounds overwhelmed in intensity by a uniform, even tone, or possibly any short sounds permeated by a long one or a long series of shorter ones, such as the roar of Broadway drowning the noise of a 'piano organ.'

Possibly this feeling might be better defined by saying that it is one which not only these few words but a whole volume would fail adequately to describe. It is a lifetime feeling which it takes all the other mental imageries to represent or all the sensations of a lifetime to equal. At the time when it originally occurred, it contained no words, of course. It was the feeling pure and simple with certain visual and possibly motor concomitant mental imagery. It was more than these words could now arouse in me. . . . .

The importance of this feeling here is the recurrence of it in the form of mental imagery two years after the real emotions occurred.

WORD IMAGERY.

In respect to the mental imagery of words or, as I shall call it, word imagery, people differ.[1] I shall confine myself to the consideration of my own word imagery, reserving for some future time the comparison of different people from this point of view. Suffice it here to say, however, that I make only individual observation of my word imagery.

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[1] The *locus classicus* being of course Stricker vs. his many French opponents.

'Violin,' whatever it suggests to the mind, and it is probably different with different people, may exist in the mind for no matter how short a time entirely apart from the instrument whose appearance, tone, feeling in our hands, etc., it suggests. Is it a word in bluish ink on white paper, or in chalk upon a blackboard, or in script, old English or Roman type, or is it the tones of some one's voice, or the articulation upon our own lips? Possibly to a deaf mute it might be also the appearance of another's lips while speaking the word. In any case, whether visual, auditory or motor, it is different from itself in any other of these forms.

The mental imagery of words forms a most interesting and important subject for study. It seems probable that when once the attention is called to the different ways in which words exist in the mind, it will be possible for the psychologist to see better the different sensory aspects of the 'thought stream' and to study them more successfully than heretofore.

If language is *not* given man 'to conceal his thoughts with' it will be easily seen that language is deceptive from the fact that very many words are associated with the imageries of more than one sense, and misunderstanding is likely to result if, *e.g.*, visualization is meant where aurilization or motilization is supplied by hearer or reader.

My attention was called, however, to the observation of even a narrower aspect of the existence of words in mental imagery by the slips of the pen and of the tongue which I have observed in myself.

I have observed in my own case that the word is almost exclusively a ***heard word*** and from several examples of *lapsus calami* and *linguæ*, particularly the former, I shall try to show that what I have observed at first hand in my own thought stream is witnessed to by objective facts of speech and handwriting.

Slips of the Tongue. When speaking, some persons, including myself, are conscious of what we are going to say just a short time before we say it. If, then, our word imagery (auditory or visual) gets too far ahead of our actual speech, sometimes there is a slip of the tongue. For instance, if we are saying 'no power whatever over,' etc., and the 'over' is heard while the 'what' is being uttered, we are, if we slip at all, more likely to say 'whatover' than 'whatever.' This is an actual *lapsus linguæ*.[1]

Slips of the Pen. As I write I hear words in my own voice in my mind's ear and, as it were, I copy them down. When the voice goes too fast I am occasionally at the same time writing one word and hearing another. The mental imagery of the latter word not yet written is sometimes strong enough to overcome the resistance which is caused by actually writing the former word and to interpolate itself. Thus in writing the phrase '*Pliny's letters*' I heard '*letters*' when I had written '*Pli*' and then started to write the word '*letters*' so that before I could stop I had written *Plil.* This, to me, seems to be a clear case of auditory mental imagery overcoming the motor ordinarily used in writing.

I wrote '*principium individuationis*' correctly yet aurilizing '*individuonis*,' thus mentally omitting two syllables which were supplied (as three letters) by the hand alone. After I had written the word I thought "I have said to myself and must have written '*individuonis*,' but found on looking back that all the letters were there. It is often the same with *remember*. I hear in my mind's ear '*remember*' (three syllables, eight letters), but write '*rember*' (six letters).

ANALYSIS OF MY WORD IMAGERY.

Very often in thinking of something to say to anyone, the words formulate themselves in my mind, *i. e.*, I hear them in my own mental voice; and then it is suggested to me either in the same voice or by a sort of mental shrinking from doing it: "No, you had better not say it now;" or, "you had better wait a few minutes." Then my mental imagery changes in a flash. I am possibly in another part of the world and have different things before my mind's eye. The sentence of words previously mentioned, however, I do not know whether I have uttered. My memory of the previous mental image is itself a mental image, and is often as strong as the previous one, and it seems as if I *may* have said the words, but I am really in doubt. Then I ask some one to be sure.

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[1] Others are 'dichomoty' (spoken) for 'dichotomy;' 'frankincelse' (melts) for frankincense; 'effecht' (on Fichte); 'interpretive' for 'interpretative:' 'denuce' for 'deduce.'

As I think these words before writing them down, I hear them mentally in my own voice, so to speak. This mental voice is generally a little ahead of the words as they are being written, and when I regard it closely seems to constitute a wholly different thing from what might be called the fountain (or, perhaps, panorama) of my other, deeper thought. The voice goes on moving by grammatical clauses and phrases, and I have usually heard the clause by the time I have written the first two or three words of it. There it stops and allows me to catch up with it. As the words are heard by my mental ear, they are carefully compared with an indescribable feeling of rightness, or wrongness; and they are tested for their capability of expressing the idea that I am trying to convey, which idea, again, is indescribable, being not words but a sort of emotional state. If they seem awkward or unfit, then other words do or do not immediately spring up of their own accord to replace the faulty word. If they do I try them one after another until I have the well-known feeling of satisfaction which is as well marked and sure as the jingle of a bell at the end of a line in typewriting. If they do not appear, I have to give up exactly that grammatical form of expressing the idea or I must resort to the dictionary to find a word that fits. In such a case my mind is for the moment an absolute blank. Verbally it is a blank, for no words will come, and unless I am describing some sight, sound or other sensation of which I retain the mental imagery before my mind's eye, it is altogether empty. If there was any mental imagery, that, of course, remains, and from that I pick up again the lost thread of my discourse.

All this time my pen watched by my external eye alone which for the time seems to act only as a censor of spelling has been scratching away and generally making mistakes. My auditory center is the seat of, or at any rate the immediate receptacle for, the verbal thought which I am about to write. This auditory center would then in turn seem to direct the hand-moving center or that part of it which governs the group of muscles which are used in writing. I believe that, immediately superintending this cheirographic center, must be a word-uttering or articulatory-motor centre which spells the words. This articulation is not however conscious, but would seem to be a little ahead of the actually appearing word and, as before stated, the audition is considerably [illegible]d of [illegible] thus:

1 [illegible] rea
2 [illegible] ak
3. - - - - - O. thy cold grey stones O, Sea!

This scheme nearly rep[illegible]nts the conditions at that period in writing Tennyson's ve[illegible] wh[illegible] the pen has reached the third word. No. 1 is the actual w[illegible]g; 2 is that part of the articulatory process which seems [illegible]o effect the spelling of the words, and 3 is the auditory form which the whole line has in my mind.

It is this word-articulatory imagery which seems to be the cause of some of the mistakes in writing, particularly those which would not be mistakes if the pronunciation were slightly altered. The substitution of a *b* for a *p* in the word 'pretty' is a mistake which is fundamentally phonetic. There is little in the shapes of the two letters that would lead them to be confounded by the hand as it writes them as would be the case were I to write an *l* for a *t* or *vice versa*. In this latter case, which has never happened to me, there is a similarity in the shapes of the letters which might lead one to be written in place of the other, but there is no phonetic similarity. In like manner there seems to be no reason why so important a letter as *n* 'graphologically' should be entirely ignored in writing a word as it has been by me in the logogram '*acient*' which I wrote for '*ancient*' and '*orage*' which I wrote for '*orange*.' Greater graphological dissimilarity with much phonetic similarity is shown by my writing '*answers*' for '*ancestors*' and in the mistake, common for me, of writing '*nts*' for '*nce*' in such words as '*correspondents*.' Also the logogram '*notwithistanding*.'

Capital letters, of course, have no phonetic existence, being purely a matter of looks. Accordingly, when I am writing very rapidly, all proper names are just as likely to come out with small initials, and more so, except in such cases of extremely well organized chirography as my signature; and my ordinary handwriting is covered with scratches made by my making such corrections, as I do just as soon as my eye comprehends that the word looks queer. Other mistakes of this nature are my writing 'to,' the word, for '2,' the figure; and such rampant phonetic spelling as *ar* for our and 'tranquil' spelt *trank*(uil).

In one case the spelt form of the word reacts on the sound of the word as I mentally hear it. *Rough* is written as two sounds, *r* and *uf*, and there is no trace in that pot-hook *g* of its guttural sound which it has when written from the auditory image of the sound of hard *g*. In writing the word *Edinburgh*, however, which otherwise I always hear in my own mental voice as if spelt *Edinboro'*, I am yet forced against my will to hear distinctly the sound of *g* guttural, as it is natural (and proper) in writing the word *Newburgh*.

In the same way I have noticed when writing out *Monstrum horrendum informe ingens*, etc., where elision of the final syllables occurs, I am constrained to hear the elided syllables, while my mental audition of the whole line, when not writing it but reading it to myself, is, of course, as the line would be read, *Monstr' horrend' inform' ingens*, etc. The auditory imagery is forcibly changed by the supereminence of the written word. Conversely, when writing German words that begin with *sch*, I generally have to think out each separate letter, because I have not the graphic sign of the equivalent English *sh* sound well enough organized. The *sh* sound is to me, in itself, neither English nor German. When written I judge it by my eye as it were for its English or German qualities. I might here say that this applies only to writing in Roman script. When writing the German script which I originally learned, the three German letters *sch* are written as a whole and from the sound.

During the time when I studied stenography I noticed in myself a tendency to *spell* phonetically (*e. g.*, *f*sician for physician) even while writing long hand.

In copying into my bibliography the title of an article—"*Ribot, Recherches*, etc., *Rev. Phil.* 38, 376–401."—I am positive that my memory image from which I copied the 401, after looking away from the page of the *Psychological Index* to the page in my notebook, was purely auditory excepting the just noticeable labio-motor. So foreign was the *look* of the number to my mind that I reassured myself that the 401 in my own handwriting was correct only by comparing the memory image of the auditory mental image of the printed '401' with the auditory image of the 'four hundred and one' in my mind's ear. And, besides, if I had a visual image (subsequently translated into auditory terms) it might have been translated into 'four hundred and first.' That it was definitely 'four hundred and one' seems to be external proof that the imagery was auditory.

It would seem probable that a word exists in the minds of all persons (though I have much testimony to the contrary) as an auditory motor image rather than as a visual image,[1] for the auditory existence of a word is the primary one (in point of evolution) and it is the only one common to all the others.

Thus if we (1) *read aloud* we have the words, (a) visually, '401'; (b) as *auditory* imagery (four hundred and one); motor, 11111. If (2) we *speak* words of our own we have *auditory* and motor imagery, and (3) if we *think* them we have *auditory* (some say motor) imagery.

If we (4) *hear* them we have *auditory* mental imagery (possibly also motor and visual).

If we *read to ourselves* (5) we have *auditory* (+ motor?).

If we *copy* (6), the imagery is largely *auditory*.

In all these cases the auditory is present. For myself the auditory element plays the greater part in all of them with the possible exception of No. 2, where in conversation the imagery is little, if at all, attended to. As the auditory imagery is the only element common to all these verbal functions it seems to be collectively the most important element. Individually it may differ, *i. e.*, different people probably have it in different proportions, but the true word is auditory alone. It is the heard word or the spoken word, and all other forms of it are but faint and unmeaning copies of it.

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[1] James cites a good case of the latter, *Psychology* Vol. II., p. 57.

Now if we copy words from print into our own handwriting it seems that the simplest and easiest way to write them is to copy either from the visual or from the auditory imagery. The graphomotor imagery, in copying from printed matter, I could not conceive. If the visual imagery is copied, there is an actual visual perception and a visual imagery in conflict, at any rate unnecessarily duplicating each other.

To retain long phrases thus in a purely visual way, even for so short a time as a few seconds, seems to me impossible. My visual memory image and my visual mental imagery has never had such minute definition as to reproduce individual words, or even single letters, in ordinary pica type. So that, in copying from my memory image, it is the auditory and not the visual which I use. With this I can hold for a short time as a unit several phrases without confusion, *e.g.*, "Liberty *and* union, now *and* forever, one *and* inseparable."

HISTORICAL NOTE ON THE PSYCHOLOGICAL DESCRIPTION OF MENTAL IMAGERY.

Literature, both artistic and philosophical, contains many references to and observations upon the *imagination*, the *phantasy* and the reproduction of sensations, from the time of Aristotle, who said that imagination is the movement which results upon actual sensation.[1]

But it is only in the latter half of the nineteenth century that it has been studied as an object of purely psychological research and not as imagination or poetry.

Much of the study of the processes of association has come more or less near to the investigation of mental imagery, but the associations collected were, of course, but words, and they were arranged according to verbal categories, *e. g.*, no attention is paid to the sense elements in which the associations *water—pail*, *candle—stick*, etc., take place, and in such reports all that has been given is the mere words and not *taste of muddy* water;

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[1] *De Anima*, III., 3, 429^a 1.

smell of new wooden pail; *touch of brass* candle-stick; *appearance of wax* candle, etc.

In the same direction have been the many investigations of the laws of memory. The psychologist's attention, however, in all these cases has been given almost entirely to explain why, on the one hand, certain things, words, etc., were remembered and why certain others were not. At most a brief 'note' is subjoined or 'remarks on the experiments' in which the subjects tell how they have happened to think of their various thoughts.

Thus, even in so recent an experiment as Binet and Henri's[1] on the memory of words and the memory of phrases, though the manner of recall was noticed it was only slightly noticed and was not studied at all. One of their subjects says the words were suggested to him partly by auditory image.

It is true, of course, that what was being studied in these papers was not the mental imagery, but was memory and association. But the fact that any observation of the modes of the mental imagery was made shows an appreciation of the interest of the study of mental imagery.

In 1843 appeared Myer's *Untersuchung über die physiologie der Nervenfaser*, with the now famous record of the author's subjectively produced after image, and in 1860 Fechner's *Psychophysik* continuing the comparison between mental imagery and after images.[2]

In 1879 Francis Galton published an article on 'Generic Images'[3] in which he tried to show that they were formed like composite photographs, by superposition on the brain of impression after impression from the sense of sight. This was followed in the next year by the article 'Mental Imagery,'[4] in which he suggested that there might be individual differences in this faculty and added to the sense of sight the other senses as likely to afford images analogous to the pictures called up in the visual imagination.

In the same year Professor Stricker of Vienna wrote[5] that his thinking in words was accompanied by a feeling in the throat and lips as of suppressed articulation, which he compared to a piano player's running his fingers over the keys of his instrument, but touching them so lightly as to make no sound on the strings.

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[1] *Année Psychologique*, I., 23 (1894).

[2] See James' *Psychology*, II., 50.

[3] *Nineteenth Century Magazine.*

[4] *Fortnightly Review.*

[5] *Studien über die Sprachvorstellungen.*

In 1881 Galton published an article on the 'Visions of Sane Persons."[1] In this he showed that five per cent. of the persons that he had examined were exceedingly vivid visualizers to the extent of having number forms.

Books dealing directly with mental imagery are to be divided into: (1) systematic treatises and (2) reports on experiments (subjective or objective), and each of these might again be subdivided into those which concern mental imagery as a whole and those treating of only a part of our mental imagery, viz.: word imagery.

I find no systematic treatise upon mental imagery as a whole.

Three books concerning the mental imagery of words are to be mentioned, those of Stricker,[2] Egger[3] and Ballet.[4]

Ballet describes in turn mental audition and vision (sensory) and mental imagery of speaking and writing (motor) from the point of view of aphasia. He cites many individual observations, but only with regard to word imagery.

The works of Egger and Stricker make an interesting contrast and illustrate a psychological misunderstanding which it took some time to explain.

Egger gives an exhaustive analysis of the word imagery from an auditory point of view, comparing his internal speech with the real sound of external speech. He says the faintest external utterance is still a sound far louder than even the loudest internal speech, and adds "*my* internal speech is the imitation of *my* voice, in timbre, intensity, peculiarities of intonation, etc. It is a simple auditory image without tactile imagery." He recognizes the other imageries, too, dividing them into: (1) *images* in the strict sense of the word or visual images; (2) internal sounds which include words as well, and (3) imageries of the other senses, smell, taste, touch, organic, muscular.

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[1] *Fortnightly Review*.

[2] Op. cit.

[3] *La parole interieure*, Paris, 1881.

[4] *Le Langage interieure*, Paris, 1888.

Stricker, in his already mentioned book, on the other hand, not only confines himself to words and the description of the mental existence of words, but sees in himself only the motor side of them. And, in addition to that, he has unfortunately been misunderstood to affirm that but one side existed. His motor imagery apparently predominates in all his thought and in his *Studien*[1] he describes very carefully his word imagery as principally the sensation of innervation of his lip and tongue and throat muscles. He says with the idea (*Vorstellung*) of every sound (*Laut*) is inseparably bound a (more or less distinct) feeling in the organs of speech. These feelings are situated in the muscles, and are similar to those experienced when we really speak the word out loud.[1] Again, "The expression 'sound' (*Laut*) is a misnomer; it implies an idea of audition which is not included in the pure idea of words.[2]

It seems that we must accept the views both of Stricker and of Egger contradictory as, at first sight, they appear. The latter says his internal speech is auditory imagery, and he is right. The former says his internal speech is articulatory-motor, and he is right.

Other writers do not stop there, however. They say to each, "You are deceiving yourself." Although Messrs. Stricker and Dodge[3] say they fail to imagine sound at all save by the rapidly vanishing memory image, and though I should say that I fail to detect any but the very slightest feeling of innervation or of muscular action or vibration in any of the organs of speech, when, in thinking silently, I look for such imagery, yet the answer will be made that the other elements are there, though they may not yet have crossed the threshold of consciousness.

Thus Netter says[4] that even visualizers, though they do not know it, must be auditory in matter of *parole interieure;* and

---

[1] Op. cit., p. 15.

[2] "Der Ausdruck 'Laut' wird zwar jetzt nicht mehr ganz passend erscheinen; er weist auf eine Schallvorstellung hin, die ja in der reinen Vorstellung der Worte nicht enthalten ist," p. 27.

[3] See below, p. 52.

[4] *La parole interieure et l'âme*, Paris, 1892,

Féré[1] draws his conclusions as follows: He was preparing a report of an autopsy and wrote *poumon 3* for *poumon droit*. He says: The motions of the hands used to write the figure 3 and the word *droit* have no similarity, but the motions necessary for the articulation of the words *trois* and *droit* have much similarity. So it seems that this *lapsus calami* was in reality a *lapsus linguæ* betrayed by writing. This shows that the mental representation of an articulate sound is accompanied by motions of the muscles specially used in articulation, and that when we represent the sound in writing we write primarily from the tongue. He adds that he cannot mentally represent an articulate sound (by which I understand the tones of the human voice) without some '*sensations musculaires*' in the speech muscles. Nevertheless, when he writes he perceives no motions in the tongue or lips, but believes that his mistake in writing shows that they exist in him.

To me it would appear just as likely that, instead of an articulatory motor center being active together with the graphomotor center, an auditory center (sensory) should guide and direct the pen. A constant motor activity not discharging into objective movement seems, to one who can hear in his mind's ear, a waste of energy. If it is true, however, as Höffding and Erdmann[2] mention, that excessive thinking in some persons so agitates the nerves of the throat as to make them hoarse; it is better to report it than to deduce it.

To say that I think in terms of speech-motor imagery, but that I am unconscious of it, contradicts completely my notions of 'type.' For to be auditory in type means not that our mental ear is good and at the same time we do not know it, but when we take occasion to inspect our internal speech, we find that by all the ear-marks we can positively recognize it as auditory. Such 'ear-marks' are the unexpected qualities of timbre which the word imagery sometimes exhibits.

Professor Baldwin contributes an interesting introspection into word imagery in his article on 'Internal Speech and Song.'[3]

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[1] *A propos d' une Lapsus Calami*, Revue Philosophique, Vol. 21, p. 546.

[2] *Psychologische Briefe*, No. 15. Cited by Stricker.

[3] *Philosophical Review*, Vol. II., p. 385.

He says: "We recognize and understand words which we are unable to pronounce aid which we have never written; this recognition must be by and of visual or auditory images. The part played by the visual and motor memories, respectively, in my own case, is seen in the fact that, when I wish to speak in any language but English, the German words come first into my mind; but when I sit down to write in a foreign language, French words invariably present themselves. This means that my German is speech motor and auditory, having been learned conversationally in Germany, while the French, which was acquired in school by reading and exercise writing, is visual and hand motor."[1]

One of the most recent works on the subject of word imagery is that of Raymond Dodge.[2] He has gone even further than Stricker in the description of these mental phenomena. He imagines words to be spoken by himself and almost invariably without auditory imagery. He gives an elaborate analysis of his motor imagery of all the letters of the English alphabet and in a table compares his own with those of Stricker.

Beaunis[3] includes some observations upon the word imagery under the name of *sensations du pensée*. The *sensations internes* are all the sensations which come to us in ways other than 'the five senses.' They are organic sensations, wants or desires for action or inaction of the various organs and muscles, and sensations which the various functions of the body give us, emotional sensations and those of a peculiar nature not included in the others. These are the sense of orientation, the magnetic sense, the meteorologic or weather sense, the sense of duration, the sense of thought, and, finally, all the pleasures and pains. As illustration of the 'sense of thought' he cites that Blake, the English artist and poet, thus called up the illustrious dead and conversed with them, soul to soul. It is surprising that he should have included word imagery and not from it passed on to other auditory-motor, then purely auditory and visual imagery, and so on.

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[1] Baldwin also cites Ballet, *op. cit.*, p. 62.

[2] *Die Motorischen Wortvorstellungen*, Halle, 1896.

[3] In *Les Sensations Internes* (1889).

The majority of the articles are reports upon investigations of the questionnaire type.

Thus Stetson and Armstrong, in articles in the *Psychological Review*, have continued with Galton's questions and with college students as subjects.

In 1891 Ribot in his *Enquête sur les idées générales*[1] reported that he had orally questioned 103 people and found that what was immediately suggested to them when they heard some general term such as animal, form, justice, relation, was (1) a concrete, visual or motor mental imagery or (2) a written or printed word or (3) a mental image of sound or articulation, sometimes both. His results, namely that about 50% of his subjects thought of 'nothing,' when abstract terms like 'cause,' 'justice,' etc., were mentioned, have been criticized as of doubtful importance. It seems to me, however, that the persons who frankly confessed to nothing probably had nothing (as I have myself many times), but the words, because there is nothing in those words which can arouse appropriate and distinctive mental imagery.

In 1895 Dugas, in an article, *Recherches experimentales sur les differents types d' images*[2] reports that he succeeded in getting ten persons to tell him what sense imagery was aroused by the words *sugar*, *lemon*, *tobacco*, *heliotrope*, *wet sponge*, *sawing marble*, etc. He regards the sense imagery, however, as the least important of the facts recorded by him.

A new note is struck by Paul Tannery. His introspection into his own mental language shows him that he has articulatory motor images when writing. He notes, however, that the word imagery does not accompany the words as they are written or spoken, but it *precedes* them, and, further, that the *will* seems to have little to do with what we say. Thus,[3] we hear ourselves speaking and we are satisfied or dissatisfied with what we have said. If we are dissatisfied, the mind interposes to make corrections. But aside from that the *will* plays no rôle, except that of regulating the intensity of the voice.

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[1] *Revue Philosophique*, Vol. 32, p. 382.

[1] *Revue Philosophique*, Vol. 39, p. 285.

[3] *Sur la parole interieure*, Revue Philosophique, 1887.

SUPPLEMENTARY.

I have now mentioned five different methods of studying mental imagery. The results gained by Experiments A and B are, in my opinion, valuable only if we bear in mind the novelty of the subject and the very meagre data which we have upon this point. Galton found that many scientific men had no visual imagery. (Does this imply no tactile, auditory, etc.?) The difficulty of getting trustworthy data by the questionnaire method is in many cases unsurmountable. The misunderstandings are numerous. One gentleman of acute mind and great learning could not imagine pictures of senses other than sight, for only sight is pictorial.

The results of Experiments C and D indicate a profitable path for finer and more scientific literary criticism. The analysis into sense content of any literary work should, by rights, take note of the borderland between the mental imagery of the five senses and that of the emotions and other internal feelings.

Experiment E, in which I have tried to analyze my own mental imagery, including word-imagery, may serve as a further comment upon and an elucidation of the difference in type of internal speech, first brought out in the discussion between Stricker and his adversaries. I hope it will also indicate some possibilities in training our introspection in the direction of careful observation of conscious mental states.

The value of conscious[1] mental imagery seems, to some, a matter of doubt. I can only say that to me it is a source of great pleasure, and in word thinking a *sine qua non*. There is a deep-rooted prejudice, however, against anything containing much of sense elements—witness the unfortunate tinge which the words sensation, sensuous, sensual, etc., have. On the other hand the training of the various sense imageries has been advocated by R. P. Halleck,[2] who advises that all the imageries of children be cultivated before the impressionable age is past. We realize, however, that it would be impossible to prove that a vivid imagery in later life was the result of training or had the slightest cause in the conscious cultivation of that faculty in youth.

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[1] Not that there is *unconscious* mental imagery, but simply to emphasize that consciousness is essential.

[2] *The Education of the Central Nervous System*, New York, 1896.

The most potent argument for a training of our sensory imagery is to me that we *realize* by imagery, and by imagery alone, the *meaning* of words, whether the words be abstract or concrete.

I will here barely mention that of which a full consideration would take too long to accomplish, and would, I think, belong more to a complete treatise on psychology than to this brief research.

The effect of the mental imagery upon the sensations may be seen, not in the comparison of a past with a present sensation, as, for instance, the size of one square of yellow paper which I saw yesterday with the one I have before me to-day, but in the strong emotional tone which, I observe, the mental imagery imparts to the sensations which I receive, while I am receiving them. The mental images which crowd upon me when listening to music or to a stirring address are those of all the five senses and of emotion too, and they in turn arouse real and present emotions which exhibit themselves physically. It is the rhetorical figure 'vision' which the orator employs to fire his audience to action and in this sense 'vision' does not exclude the imagery of the other senses. It is chiefly visual elements which compose my mental imagery while listening to Beethoven's Pastoral Symphony, while all the others contribute; for there is not only the sight of the trees of the forest with patches of sunlight, but the soft, cool, moist touch of the moss, the pleasant warmth of the sun, the fragrance of the leaf mold and fresh air of the stream and the oppression of the atmosphere before the storm.

I have found that what the mental imagery brings to the sensation produces the effect of two sensations at once. The half hidden, the 'suggestive,' is, too, much more potent than what is entirely laid bare. "The modest charm of not too much, part seen, imagined part." I seem to have an increased power, or the emotions which crescent power produces, when I see through or behind any thought fabric or philosophical integument.

The metaphor and simile in literature put one mental image before the other so that, as it were, we see both at once, *e. g.*, "Now lies the earth all Danaë to the stars." I think that in this alone we derive our chief pleasure, not from the fact that Tennyson has here expressed an original thought in beautiful language, but from the visual imagery of the stars as a shower of gold into which Jupiter transformed himself.

I will instance only the following as what appropriate mental imagery, and only that, can make comprehensible:

> 'Mid flowers fair-heaped there stood a bowl
>   With water. She therein
> Through eddying bubbles slid a cup
> And offered it, being risen up,
> Close to her sister's mouth to sup.—Rossetti's *Bride's Prelude*.

This very good example of the visual element in the painter-poet's literary work calls for a very vivid visualization on the part of the reader to make it beautiful to him.

A Belgian writer, Paul de Reul, thus develops the mental imagery in the following lines from Rossetti's *Jenny*:

> Lazy, lanquid, laughing Jenny,
> Fond of a kiss and fond of a guinea.

He says of this: "Le retour des mêmes consonnes est euphonique; il est en suite expressif, car le L exprime la voluptueuse langueur, *le F nous fait voir deux levres se tendant pour un baiser, le quel devient audible dans 'kiss.*[1]"

If it be true that the arousing of vivid mental image in the mind be of value in our education as an awakening of the finer emotions, as I believe it is, then we have, as psychologists, the greatest possible need to investigate the two following problems which in my present essay could be no more than indicated.

1st. Is the faculty of imagery susceptible to training? and, if not,

2d. Can we tell by outward signs, by objective experiments, by the questionnaire, oral and written, or by the simple reaction times to various stimuli (of sight, sound, touch, etc.), whether a given person possesses the faculty and in what degree, and in what direction?

[1] *La Langue et le Style*, Revue de l' Université de Bruxelles, No. 10.

I confess that my own experiments upon other persons have as yet given me but little result. I have already mentioned the difficulties I encountered in my questionnaire research (see p. 7) among college students, among artists and in my oral examination of the latter. Words are so spontaneous that what is behind them is often misjudged and prejudice against confessing anything like 'visions' among 'sane persons' tends to conceal truth, and the reverie or undirected play of the imagination is so little respected as to be seldom brought forth to the light of science.

I believe that if the mental imagery is susceptible to training it will appear so in that part of us where we receive most and earliest mental training, viz.: in our reading, writing and arithmetic.

With the realization of the various types of word imagery and a discovery of what they mean we shall be on the road to a better understanding of words, language, philosophy.

Is a single sense imagery of an object generally occupying the stream of consciousness at any given time? Is it always, or only sometimes, a sense complex? If words exclusively filled the stream they might for a long time satisfy, but could they be perpetually adequate? To what extent would we go in relying solely upon words, before our minds should demand some sense imagery content, to correspond to the flight of words? If words mean both words and things, if they represent both words and other mental imagery, then a full realization of this will have to precede a complete and consistent philosophy of life.

Whether words which depart more and more from a meaning, that can be called mental imagery, can be used in philosophy is a question which cannot be discussed here. Philosophy might be solely a word system and yet have a positive value. But that words do depart more and more from a mental imagery meaning and approach nearer and nearer to a *flatus vocis* is something that, though recognized by one party of linguists, can be fully illustrated only by a thorough research into mental imagery.

That we work as well with the mental imagery as with actual words is shown by the way most persons learn to read a foreign language to the exclusion of writing or speaking it. Mr. Babbitt, of the Germanic department of Columbia University, has shown[1] that one gets a working knowledge of a language sufficiently well by learning to read it.

In learning merely to read a foreign language we are paying more attention to the language itself than to the mental imagery aroused in us by the French, German or Italian, etc., words. To learn the language is something more, viz., to speak it and to write it, which is an accomplishment, like playing the piano, to be acquired if there is time and opportunity. In learning not merely to read the language, but to speak it and to [illegible] it, we [illegible] we are constantly practicing in our ow[illegible] imagery of our own minds in words. [illegible] is lea[illegible] the [illegible]uage; the other is learning fo[illegible]

[illegible] realize [illegible]een languages in respect to mental [illegible] possibility that their effect even upon [illegible]as not yet been fully stated. Visualization as [illegible]ms of some English psychologists has [illegible][2] We may find the motor sense as th[illegible]hilosophies.

What may [illegible] of types in word imagery? Does it, perhaps, indicate that the signification of words may have a *motor* value, as it were, for some people and a merely auditory *value* for others? We observe that some people are laconic, others verbose. Possibly strength of argument may seem to the latter to reside in length of sentence and power of innervating the speech muscles in long-continued inundations of syllogisms. We remember the discussions[3] between Socrates and Protagoras about whether they should talk in short sentences in Socrates' fashion or whether Protagoras should be allowed his long flights of argument. And we remember, too, that Socrates *heard* the voice of his Dæmon. So, possibly, if one is auditory-linguistic he should never enter into an argument with the motor-linguistic person, as, on all topics except the most concrete facts, either will inevitably fail, completely, to understand the other.

---

[1] In a paper entitled 'Common Sense in Teaching Modern Languages, in 'Methods of Teaching Modern Languages.' 2d Ed., Boston, 1897.

[2] By A. Fraser. *Visualization as a Chief Source of the Psychology of Hobbes, Locke*, etc., *Am. Jour. Psy.*, Dec., 1891.

[3] *Protagoras*, 334 C.

The spontaneous quality of our mental imagery is finally to be insisted upon. I can, of course, but refer to the words of Tannery (p. 53) and add to them my own testimony. My mental images come and go as they please, and I cannot persuade myself that I have the least control over them. When I think, the words in which I think come to me. I may be more responsible for the words I speak aloud than for those which come to my mind's ear, for the latter, it would seem, may be either spoken or left unsaid. At any rate, they are, for a time, before a sort of tribunal which judges them for their intellectual and moral worth. Then, if they pass unchallenged by this tribunal they are uttered. This challenge, however, or inhibition comes in the shape sometimes of words such as "it would be better not to say that" sometimes of a feeling of restraint. In either case whether words or feelings the inhibition comes as it were from without. It has no more 'warmth and intimacy' than the other words and feelings. It is not ours any more, therefore, than the other. Neither is ours.

The objectification of these two sets of words has been poetically told by Tennyson in his 'Two Voices.'

The deterministic implication will be obvious, I believe. If our thoughts are not directly under our control much less are our actions.

Further, we might say that the mental images themselves constitute the motives, the springs of action, for all that we do.

The relation of the mental imagery to the idea, in the general sense; the nature of abstract thought (is it emotion imagery?) with a better definition of abstract terms—these are some of the problems which I believe will be elucidated by a better knowledge of the concrete sensory thought processes, through the study of the mental imagery of all the ten types, including word-imagery.

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TABLE OF CONTENTS.

PAGE.
Introduction, ... 1
Description of Apparatus, ... 8
Experiments with Cats, ... 12
“ “ Dogs, ... 31
“ “ Chicks, ... 34
Reasoning or Inference, ... 39
Imitation, ... 47
“ in Chicks, ... 51
“ “ Cats, ... 54
“ “ Dogs, ... 59
The Mental Fact in Association, ... 65
Association by Similarity and the Formation of Concepts, . . 78
Criticism of Previous Theories, ... 85
Delicacy of Association, ... 87
Complexity of Associations, ... 91
Number of Associations, ... 93
Permanence of Associations, ... 95
Inhibition of Instincts by Habit, ... 99
Attention, ... 101
The Social Consciousness of Animals, ... 102
Interaction, ... 103
Applications to Pedagogy, Anthropology, etc., ... 104
Conclusion, ... 107

ANIMAL INTELLIGENCE; AN EXPERIMENTAL STUDY OF THE ASSOCIATIVE PROCESSES IN ANIMALS.

This monograph is an attempt at an explanation of the nature of the process of association in the animal mind. Inasmuch as there have been no extended researches of a character similar to the present one either in subject-matter or experimental method, it is necessary to explain briefly its standpoint.

Our knowledge of the mental life of animals equals in the main our knowledge of their sense-powers, of their instincts or reactions performed without experience, and of their reactions which are built up by experience. Confining our attention to the latter we find it the opinion of the better observers and analysts that these reactions can all be explained by the ordinary associative processes without aid from abstract, conceptual, inferential thinking. These associative processes then, as present in animals' minds and as displayed in their acts, are my subject-matter. Any one familiar in even a general way with the literature of comparative psychology will recall that this part of the field has received faulty and unsuccessful treatment. The careful, minute, and solid knowledge of the sense-organs of animals finds no counterpart in the realm of associations and habits. We do not know how delicate or how complex or how permanent are the possible associations of any given group of animals. And although one would be rash who said that our present equipment of facts about instincts was sufficient or that our theories about it were surely sound, yet our notion of what occurs when a chick grabs a worm are luminous and infallible compared to our notion of what happens when a kitten runs into the house at the familiar call. The reason that they have satisfied us as well as they have is just that they are so vague. We say that the kitten associates the sound 'kitty kitty' with the experience of nice milk to drink, which does very well for a common-sense answer. It also suffices as a rebuke to those who would have the kitten ratiocinate about the matter, but it fails to tell what real mental content is present. Does the kitten feel “*sound of call, memory-image of milk in a saucer in the kitchen, thought of running into the house, a feeling, finally, of ‘I will run in’?*” Does he perhaps feel only the sound of the bell and an impulse to run in, similar in quality to the impulses which make a tennis player run to and fro when playing? The word association may cover a multitude of essentially different processes, and when a writer attributes anything that an animal may do to association his statement has only the negative value of eliminating reasoning on the one hand and instinct on the other. His position is like that of a zoölogist who should to-day class an animal among the ‘worms.’ To give to the word a positive value and several definite possibilities of meaning is one aim of this investigation.

The importance to comparative psychology in general of a more scientific account of the association-process in animals is evident. Apart from the desirability of knowing all the facts we can, of whatever sort, there is the especial consideration that these associations and consequent habits have an immediate import for biological science. In the higher animals the bodily life and preservative acts are largely directed by these associations. They, and not instinct, make the animal use the best feeding grounds, sleep in the same lair, avoid new dangers and profit by new changes in nature. Their higher development in mammals is a chief factor in the supremacy of that group. This, however, is a minor consideration. The main purpose of the study of the animal mind is to learn the development of mental life down through the phylum, to trace in particular the origin of human faculty. In relation to this chief purpose of comparative psychology the associative processes assume a rôle predominant over that of sense-powers or instinct, for in a study of the associative processes lies the solution of the problem. Sense-powers and instincts have changed by addition and supersedence, but the cognitive side of consciousness has changed not only in quantity but also in quality. Somehow out of these associative processes have arisen human consciousnesses with their sciences and arts and religions. The association of ideas proper, imagination, memory, abstraction, generalization, judgment, inference, have here their source. And in the metamorphosis the instincts, impulses, emotions and sense-impressions have been transformed out of their old natures. For the origin and development of human faculty we must look to these processes of association in lower animals. Not only then does this department need treatment more, but promises to repay the worker better.

Although no work done in this field is enough like the present investigation to require an account of its results, the *method* hitherto in use invites comparison by its contrast and, as I believe, by its faults. In the first place, most of the books do not give us a psychology, but rather a *eulogy*, of animals. They have all been about animal *intelligence*, never about animal *stupidity*. Though a writer derides the notion that animals have reason, he hastens to add that they have marvellous capacity of forming associations, and is likely to refer to the fact that human beings only rarely reason anything out, that their trains of ideas are ruled mostly by association, as if, in this latter, animals were on a par with them. The history of books on animals' minds thus furnishes an illustration of the well-nigh universal tendency in human nature to find the marvelous wherever it can. We wonder that the stars are so big and so far apart, that the microbes are so small and so thick together, and for much the same reason wonder at the things animals do. They used to be wonderful because of the mysterious, God-given faculty of instinct, which could almost remove mountains. More lately they have been wondered at because of their marvellous mental powers in profiting by experience. Now imagine an astronomer tremendously eager to prove the stars as big as possible, or a bacteriologist whose great scientific desire is to demonstrate the microbes to be very, very little! Yet there has been a similar eagerness on the part of many recent writers on animal psychology to praise the abilities of animals. It cannot help leading to partiality in deductions from facts and more especially in the choice of facts for investigation. How can scientists who write like lawyers, defending animals against the charge of having no power of rationality, be at the same time impartial judges on the bench? Unfortunately the real work in this field has been done in this spirit. The level-headed thinkers who might have won valuable results have contented themselves with arguing against the theories of the eulogists. They have not made investigations of their own.

In the second place the facts have generally been derived from anecdotes. Now quite apart from such pedantry as insists that a man's word about a scientific fact is worthless unless he is a trained scientist, there are really in this field special objections to the acceptance of the testimony about animals' intelligent acts which one gets from anecdotes. Such testimony is by no means on a par with testimony about the size of a fish or the migration of birds, etc. For here one has to deal not merely with ignorant or inaccurate testimony, but also with prejudiced testimony. Human folk are as a matter of fact eager to find intelligence in animals. They like to. And when the animal observed is a pet belonging to them or their friends, or when the story is one that has been told as a story to entertain, further complications are introduced. Nor is this all. Besides commonly misstating what facts they report, they report only such facts as show the animal at his best. Dogs get lost hundreds of times and no one ever notices it or sends an account of it to a scientific magazine. But let one find his way from Brooklyn to Yonkers and the fact immediately becomes a circulating anecdote. Thousands of cats on thousands of occasions sit helplessly yowling, and no one takes thought of it or writes to his friend, the professor; but let one cat claw at the knob of a door supposedly as a signal to be let out, and straightway this cat becomes the representative of the cat-mind in all the books. The unconscious distortion of the facts is almost harmless compared to the unconscious neglect of an animal's mental life until it verges on the unusual and marvelous. It is as if some denizen of a planet where communication was by thought-transference, who was surveying humankind and reporting their psychology, should be oblivious to all our inter-communication save such as the psychical-research society has noted. If he should further misinterpret the cases of mere coincidence of thoughts as facts comparable to telepathic communication, he would not be more wrong than some of the animal psychologists. In short, the anecdotes give really the *abnormal* or *super-normal* psychology of animals.

Further, it must be confessed that these vices have been only ameliorated, not obliterated, when the observation is first-hand, is made by the psychologist himself. For as men of the utmost scientific skill have failed to prove good observers in the field of spiritualistic phenomena,[1] so biologists and psychologists before the pet terrier or hunted fox often become like Samson shorn. They, too, have looked for the intelligent and unusual and neglected the stupid and normal.

Finally, in all cases, whether of direct observation or report by good observers or bad, there have been three other defects. Only a single case is studied, and so the results are not necessarily true of the type; the observation is not repeated, nor are the conditions perfectly regulated; the previous history of the animal in question is not known. Such observations may tell us, if the observer is perfectly reliable, that a certain thing takes place, but they cannot assure us that it will take place universally among the animals of that species, or universally with the same animal. Nor can the influence of previous experience be estimated. All this refers to means of getting knowledge about what animals *do*. The next question is, "What do they *feel?*" Previous work has not furnished an answer or the material for an answer to this more important question. Nothing but carefully designed, crucial experiments can. In abandoning the old method one ought to seek above all to replace it by one which will not only tell more accurately *what they do*, and give the much-needed information *how they do it*, but also inform us *what they feel* while they act.

To remedy these defects experiment must be substituted for observation and the collection of anecdotes. Thus you immediately get rid of several of them. You can repeat the conditions at will, so as to see whether or not the animal's behavior is due to mere coincidence. A number of animals can be subjected to the same test, so as to attain typical results. The animal may be put in situations where its conduct is especially instructive. After considerable preliminary observation of animals' behavior under various conditions, I chose for my general method one which, simple as it is, possesses several other marked advantages besides those which accompany experiment of any sort. It was merely to put animals when hungry in enclosures from which they could escape by some simple act, such as pulling at a loop of cord, pressing a lever, or stepping on a platform. (A detailed description of these boxes and pens will be given later.) The animal was put in the enclosure, food was left outside in sight, and his actions observed. Besides recording his general behavior, special notice was taken of how he succeeded in doing the necessary act (in case he did succeed), and a record was kept of the time that he was in the box before performing the successful pull, or clawing, or bite. This was repeated until the animal had formed a perfect association between the sense-impression of the interior of that box and the impulse leading to the successfnl movement. When the association was thus perfect, the time taken to escape was, of course, practically con stant and very short.

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[1] I do not mean that scientists have been too credulous with regard to spiritualism, but am referring to the cases where ten or twenty scientists have been sent to observe some trick-performance by a spiritualistic 'medium,' and have all been absolutely confident that they understood the secret of its performance, *each of them giving a totally different explanation.*

If, on the other hand, after a certain time the animal did not succeed, he was taken out, but *not fed.* If, after a sufficient number of trials, he failed to get out, the case was recorded as one of complete failure. Enough different sorts of methods of escape were tried to make it fairly sure that association in general, not association of a particular sort of impulse, was being studied. Enough animals were taken with each box or pen to make it sure that the results were not due to individual peculiarities. None of the animals used had any previous acquaintance with any of the mechanical contrivances by which the doors were opened. So far as possible the animals were kept in a uniform state of hunger, which was practically utter hunger. That is, no cat or dog was experimented on when the experiment involved any important question of fact or theory, unless I was sure that his motive was of the standard strength. With chicks this is not practicable, on account of their delicacy. But with them dislike of loneliness acts as a uniform motive to get back to the other chicks. Cats (or rather kittens), dogs and chicks were the subjects of the experiments. All were apparently in excellent health, save an occasional chick.

By this method of experimentation the animals are put in situations which call into activity their mental functions and permit them to be carefully observed. One may, by following it, observe personally more intelligent acts than are included in any anecdotal collection. And this actual vision of animals in the act of using their minds is far more fruitful than any amount of histories of what animals have done without the history of how they did it. But besides affording this opportunity for purposeful and systematic observation, our method is valuable because it frees the animal from any influence of the observer. The animal's behavior is quite independent of any factors save its own hunger, the mechanism of the box it is in, the food outside, and such general matters as fatigue, indisposition, etc. Therefore the work done by one investigator may be repeated and verified or modified by another. No personal factor is present save in the observation and interpretation. Again, our method gives some very important results which are quite uninfluenced by *any* personal factor in any way. The curves showing the progress of the formation of associations, which are obtained from the records of the times taken by the animal in successive trials, are facts which may be obtained by any observer who can tell time. They are absolute, and whatever can be deduced from them is sure. So also the question of whether an animal does or does not form a certain association requires for an answer no higher qualification in the observer than a pair of eyes. The literature of animal psychology shows so uniformly and often so sadly the influence of the personal equation that any method which can partially eliminate it deserves a trial.

Furthermore, although the associations formed are such as could not have been previously experienced or provided for by heredity, they are still not too remote from the animal's ordinary course of life. They mean simply the connection of a certain act with a certain situation and resultant pleasure, and this general type of association is found throughout the animal's life normally. The muscular movements required are all such as might often be required of the animal. And yet it will be noted that the acts required are nearly enough like the acts of the anecdotes to enable one to compare the results of experiment by this method with the work of the anecdote school. Finally, it may be noticed that the method lends itself readily to experiments on imitation.

We may now start in with the description of the apparatus and of the behavior of the animals.[1]

DESCRIPTION OF APPARATUS.

The shape and general apparatus of the boxes which were used for the cats is shown by the accompaning drawing of box K. Unless special figures are given, it should be understood that each box is approximately 20 inches long by 15 broad by 12 high. Except where mention is made to the contrary, the door was pulled open by a weight attached to a string which ran over a pulley and was fastened to the door, just as soon as the

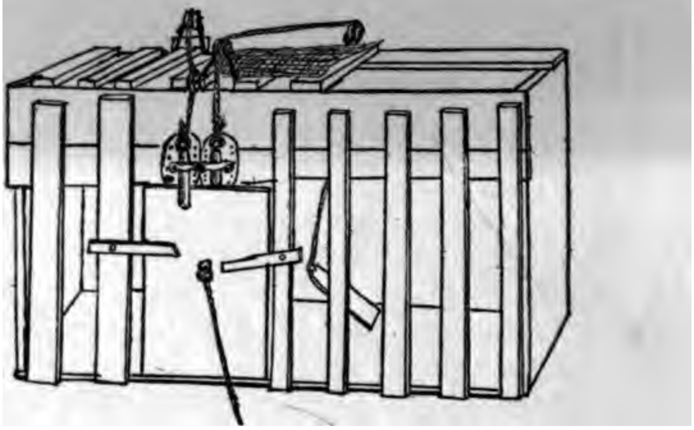

FIG. 1.

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[1] The experiments now to be described were for the most part made in the Psychological Laboratory of Columbia University during the year '97–'98, but a few of them were made in connection with a general preliminary investigation of animal psychology undertaken at Harvard University in the previous year.

animal loosened the bolt or bar which held it. Especial care was taken not to have the widest openings between the bars at all near the lever, or wire-loop, or what not, which governed the bolt on the door. For the animal instinctively attacks the large openings first, and if the mechanism which governs the opening of the door is situated near one of them the animal's task is rendered easier. You do not then get the association process so free from the helping hand of instinct as you do if you make the box without reference to the position of the mechanism to be set up within it. These various mechanisms are so simple that a verbal description will suffice in most cases. The facts which the reader should note are the nature of the movement which the cat had to make, the nature of the object at which the movement was directed, and the position of the object in the box. In some special cases attention will also be called to the force required. In general, however, that was very slight (20 to 100 grams if applied directly). The various boxes will be designated by capital letters.

A. A string attached to the bolt which held the door ran up over a pulley on the front edge of the box, and was tied to a wire loop (2½ inches diameter) hanging 6 inches above the floor in front center of box. Clawing or biting it, or rubbing against it even, if in a certain way, opened the door. We may call this box A '*O at front.*'

B. A string attached to the bolt ran up over a pulley on the front edge of the door, then across the box to another pulley screwed into the inside of the back of the box 1¼ inches below the top, and passing over it ended in a wire loop (3 inches in diameter) 6 inches above the floor in back center of box. Force applied to the loop or *to the string* as it ran across the top of the box between two bars would open the door. We may call B '*O at back.*'

B1. In B1 the string ran outside the box, coming down through a hole at the back, and was therefore inaccessible and invisible from within. Only by pulling the loop could the door be opened. B1 may be called '*O at back 2nd.*'

C. A door of the usual position and size (as in Fig. 1) was kept closed by a wooden button 3½ inches long, ⅞ inch wide,

¼ inch thick. This turned on a nail driven into the box ½ inch above the middle of the top edge of the door. The door would fall inward as soon as the button was turned from its vertical to a horizontal position. A pull of 125 grams would do this if applied sideways at the lowest point of the button 2¼ inches below its pivot. The cats usually clawed the button round by downward pressure on its top edge, which was 1¼ inches above the nail. Then, of course, more force was necessary. C may be called '*Button*.'

D. The door was in the extreme right of the front. A string fastened to the bolt which held it ran up over a pulley on the top edge and back to the top edge of the back side of the box (3 inches in from the right side) and was there firmly fastened. The top of the box was of wire screening and arched over the string ¾ inch above it along its entire length. A slight pull on the string anywhere opened the door. This box was 20 × 16, but a space 7 × 16 was partitioned off at the left by a wire screen. D may be called '*String*.'

D1 was the same box as B, but had the string fastened firmly at the back instead of running over a pulley and ending in a wire loop. We may call it '*String 2nd*.'

E. A string ran from the bolt holding the door up over a pulley and down to the floor outside the box, where it was fastened 2 inches in front of the box and 1½ inches to the left of the door (looking from the inside). By poking a paw out between the bars and pulling this string inward the door would be opened. We may call E '*String outside*.'

In F the string was not fastened to the floor but ended in a loop 2½ inches in diameter which could be clawed down so as to open the door. Unless the pull was in just the right direction, the string was likely to catch on the pulley. This loop hung 3 inches above the floor, and 1¾ inches in front of the box. We may call F '*String outside unfastened*.'

G was a box 29×20½×22½, with a door 29×12 hinged on the left side of the box (looking from within), and kept closed by an ordinary thumb-latch placed 15 inches from the floor. The remainder of the front of the box was closed in by wooden bars. The door was a wooden frame covered with screening.

It was *not* arranged so as to open as soon as the latch was lifted, but required a force of 400 grams, even when applied to the best advantage. The bar of the thumb-latch, moreover, would fall back into place again unless the door were pushed out at least a little. The top of this box was not of bars or screening, but solid. We may call G '*Thumb-latch.*'

H was, except for the opening where the door was situated, a perfectly solid and dark box. In the front was cut an opening about 9 x 7 inches. A wooden frame covered with wire netting hung in front of this. It was fastened to the box only by a screw through the middle of the frame's top piece, and could therefore be pushed to either side so as to permit escape from the box if it were prevented from swinging back into place.

I was a box 12 x 14 x 23. The door was 8 inches wide, 4 high, and hinged at the left side. It was held closed by a wooden bar which moved easily on a pivot and which could be pushed up by another bar which projected 5 inches into the box. This second bar was pivoted so that downward pressure on it pushed the first bar up and let the door swing open. The second bar entered the box at a point 4 inches above the floor and 2½ inches in from the right side of the box. In its normal position its inner end was 5½ inches above the floor. A depression of 2 inches at that end was necessary to open the door. Of course, nearer the pivot a shorter depression would do. The front of the box was closed by bars, but the rest by solid boards. We may call I '*Lever.*'

J was the same as B except that the door was not opened by a weight as soon as the bolt was pulled up. On the contrary, the door was held closed by a small piece of board (4 x 3½ x ¾ thick) placed against it outside. After pulling the loop at the back the cat had to knock down this support and push the door open. We may call J '*Double.*'

K was a box arranged so that three separate acts were required to open the door, which was held by two bolts at the top and two bars outside. One of the bolts was connected with a platform in the back center of the box so that depressing the platform raised the bolt. The other was raised by a string which ran up over a pulley in the front, across the box 1 inch above the bars, over a pulley near the corner of the box, and down to the floor, where it was fastened. Pulling on this string, either by clawing at it where it was running vertically from the last pulley to the floor, or by putting the paw out between the bars which covered the top of the box, and clawing the string downwards, would raise the bolt. If both bolts were raised and *either* bar was pushed up or down far enough to be out of the way, the cat could escape. K, or 'Triple,' as it may be called, is the box reproduced in Figure 1.

L was a box that also required three acts to open the door. It was a combination of A (O at front), D (string), I (lever). The lever or bar to be depressed was 2 inches to the right of the door, which was in the front center. The string to be clawed or bitten ran from front center to back center 1 inch below the top of the box.

Z was a box with back and sides entirely closed, with front and top closed by bars and screening, with a small opening in the left front corner. A box was held in front of this and drawn away when the cats happened to lick themselves. Thus escape and food followed always upon the impulse to lick themselves, and they soon would immediately start doing so as soon as pushed into the box. The same box was used with the impulse changed to that for scratching themselves. The size of this box was 15 x 10 x 16.

EXPERIMENTS WITH CATS.

In these various boxes were put cats from among the following. I give approximately their ages while under experiment.

No. 1. 8–10 months.
No. 2. 5–7 months.
No. 3. 5–11 months.
No. 4. 5–8 months.
No. 5. 5–7 months.
No. 6. 3–5 months.
No. 7. 3–5 months.
No. 8. 6–6½ months.
No. 10. 4–8 months.

No. 11. 7–8 months.
No. 12. 4–6 months.
No. 13. 18–19 months.

The behavior of all but 11 and 13 was practically the same. When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening; it claws and bites at the bars or wire; it thrusts its paws out through any opening and claws at everything it reaches; it continues its efforts when it strikes anything loose and shaky; it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigor with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly. With 13, an old cat, and 11, an uncommonly sluggish cat, the behavior was different. They did not struggle vigorously or continually. On some occasions they did not even struggle at all. It was therefore necessary to let them out of some box a few times, feeding them each time. After they thus associate climbing out of the box with getting food, they will try to get out whenever put in. They do not, even then, struggle so vigorously or get so excited as the rest. In either case, whether the impulse to struggle be due to an instinctive reaction to confinement or to an association, it is likely to succeed in letting the cat out of the box. The cat that is clawing all over the box in her impulsive struggle will probably claw the string or loop or button so as to open the door. And gradually all the other non-successful impulses will be stamped out and the particular impulse leading to the successful act will be stamped in by the resulting pleasure, until, after many trials, the cat will, when put in the box, immediately claw the button or loop in a definite way.

The starting point for the formation of any association in these cases, then, is the set of instinctive activities which are aroused when a cat feels discomfort in the box either because of confinement or a desire for food. This discomfort, plus the sense-impression of a surrounding, confining wall, expresses itself prior to any experience, in squeezings, clawings, bitings, etc. From among these movements one is selected by success. But this is the starting point only in the case of the first box experienced. After that the cat has associated with the feeling of confinement certain impulses which have led to success more than others and are thereby strengthened. A cat that has learned to escape from A by clawing has when put into C or G a greater tendency to claw at things than it instinctively had at the start, and a less tendency to squeeze through holes. A very pleasant form of this decrease in instinctive impulses was noticed in the gradual cessation of howling and mewing. However, the useless instinctive impulses die out slowly, and often play an important part even after the cat has had experience with six or eight boxes. And what is important in our previous statement, namely, that the activity of an animal when first put into a new box is not directed by any appreciation of *that* box's character, but by certain general impulses to acts, is not affected by this modification. Most of this activity is determined by heredity; some of it, by previous experience.

My use of the words *instinctive* and *impulse* may cause some misunderstanding unless explained here. Let us, throughout this book, understand by instinct any reaction which an animal makes to a situation *without experience*. It thus includes unconscious as well as conscious acts. Any reaction, then, to totally new phenomena, when first experienced, will be called instinctive. Any impulse then felt will be called an instinctive impulse. Instincts include whatever the nervous system of an animal, as far as inherited, is capable of. My use of the word will, I hope, everywhere make clear what fact I mean. If the reader gets the fact meant in mind it does not in the least matter whether he would himself call such a fact instinct or not. Any one who objects to the word may substitute 'hocus-pocus' for it wherever it occurs. The definition here made will not be used to prove or disprove any theory, but simply as a signal for the reader to imagine a certain sort of fact.

The word *impulse* is used against the writer's will, but there is no better. Its meaning will probably become clear as the reader finds it in actual use, but to avoid misconception at any time I will state now that *impulse* means the consciousness accompanying a muscular innervation *apart* from *that feeling of the act which comes from seeing oneself move, from feeling one's body in a different position, etc.* It is the *direct feeling of the doing* as distinguished from the *idea of the act done* gained through eye; etc. For this reason I say 'impulse *and* act' instead of simply 'act.' Above all, it must be borne in mind that by impulse I never mean the *motive* to the act. In popular speech you may say that hunger is the impulse which makes the cat claw. That will never be the use here. The word *motive* will always denote that sort of consciousness. Any one who thinks that the act ought not to be thus subdivided into impulse and deed may feel free to use the word *act* for *impulse* or *impulse and act* throughout, if he will remember that the act in this aspect of being felt as to be done or as doing is in animals the important thing, is the thing which gets associated, while the act as done, as viewed from outside, is a secondary affair. I prefer to have a separate word, impulse, for the former, and keep the word act for the latter, which it commonly means.

Starting, then, with its store of instinctive impulses, the cat hits upon the successful movement, and gradually associates it with the sense-impression of the interior of the box until the connection is perfect, so that it performs the act as soon as confronted with the sense-impression. The formation of each association may be represented graphically by a time-curve. In these curves lengths of one millimeter along the abscissa represent successive experiences in the box, and heights of one millimeter above it each represent ten seconds of time. The curve is formed by joining the tops of perpendiculars erected along the abscissa 1 mm. apart (the first perpendicular coinciding with the y line), each perpendicular representing the time the cat was in the box before escaping. Thus, in Fig. 2 on page 18 the curve marked 12 in A shows that, in 24 experiences or trials in box A, cat 12 took the following times to perform the act, 160 sec., 30 sec., 90 sec., 60, 15, 28, 20, 30, 22, 11, 15, 20, 12, 10, 14, 10, 8, 8, 5, 10, 8, 6, 6, 7. A short vertical line below the abscissa denotes that an interval of approximately 24 hours elapsed before the next trial. Where the interval was longer it is designated by a figure 2 for two days, 3 for three days, etc. If the interval was shorter the number of hours is specified by 1 hr., 2 hrs., etc. In many cases the animal failed in some trial to perform the act in ten or fifteen minutes and was then taken out by me. Such failures are denoted by a break in the curve either at its start or along its course. In some cases there are short curves after the main ones. These, as shown by the figures beneath, represent the animal's mastery of the association after a very long interval of time, and may be called memory curves. A discussion of them will come in the last part of the book.

The time-curve is obviously a fair representation of the progress of the formation of the association, for the two essential factors in the latter are the disappearance of all activity save the particular sort which brings success with it, and perfection of that particular sort of act so that it is done precisely and at will. Of these the second is, on deeper analysis, found to be a part of the first; any clawing at a loop except the particular claw which depresses it is theoretically a useless activity. If we stick to the looser phraseology, however, no harm will be done. The combination of these two factors is inversely proportional to the time taken, provided the animal surely wants to get out at once. This was rendered almost certain by the degree of hunger. Theoretically a perfect association is formed when both factors are perfect,—when the animal, for example, does nothing but claw at the loop, and claws at it in the most useful way for the purpose. In some cases (*e. g.*, 2 in K on page 26) neither factor ever gets perfected in a great many trials. In some cases the first factor does but the second does not, and the cat goes at the thing not always in the desirable way. In all cases there is a fraction of the time which represents getting oneself together after being dropped in the box, and realizing where one is. But for our purpose all these matters count little, and we may take the general slope of the curve as representing very fairly the progress of the association. The slope of any particular part of it may be due to accident. Thus, very often the second experience may have a higher time-point than the first, because the first few successes may all be entirely due to accidentally hitting the loop, or whatever it is, and whether the accident will happen sooner in one trial than another is then a matter of chance. Considering the general slope, it is, of course, apparent that a gradual descent—say, from initial times of 300 sec. to a constant time of 6 or 8 sec. in the course of 20 to 30 trials—represents a difficult association, while an abrupt descent, say in 5 trials, from a similar initial height, represents a very easy association. Thus, 2 in Z, on page 23, is a hard, and 1 in I, on page 22, an easy association.

In boxes A, C, D, E, I, 100 % of the cats given a chance to do so hit upon the movement and formed the association. The following table shows the results where some cats failed.

	No. cats tried.	*No. cats failed.*
F	5	4
G	8	5
H	9	2
J	5	2
K	5	2

The time-curves follow. By referring to the description of apparatus they will be easily understood. Each mm. along the abscissa represents one trial. Each mm. above it represents 10 seconds.

These time-curves show, in the first place, what associations are easy for an animal to form, and what are hard. The act must be one which the animal will perform in the course of the activity which its inherited equipment incites or its previous experience has connected with the sense-impression of a box's interior. The oftener the act naturally occurs in the course of such activity, the sooner it will be performed in the first trial or so, and this is one condition, sometimes, of the ease of forming the association. For if the first few successes are five minutes apart the influence of one may nearly wear off before the next, while if they are forty seconds apart the influences may get summated. But this is not the only or the main condition of the celerity with which an association may be formed. It depends also on the amount of attention given to the act. An act of

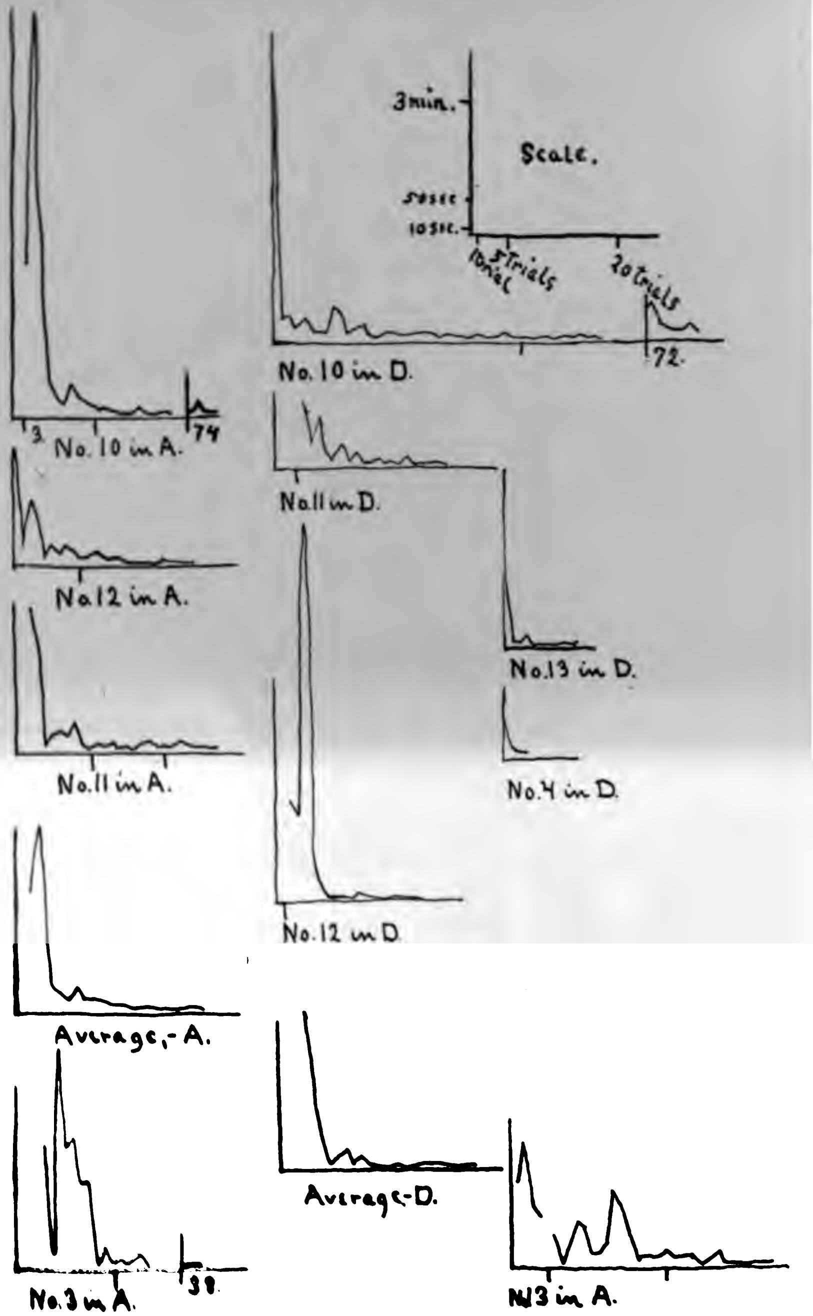

3 min.
Scale.
50 sec.
10 sec.
10 sec.
5 Trials
20 Trials
No. 10 in D.
72.
3
74
No. 10 in A.
No. 11 in D.
No. 12 in A.
No. 13 in D.
No. 11 in A.
No. 4 in D.
No. 12 in D
Average, - A.
Average-D.
No. 3 in A.
38.
No. 13 in A.

FIG. 2.

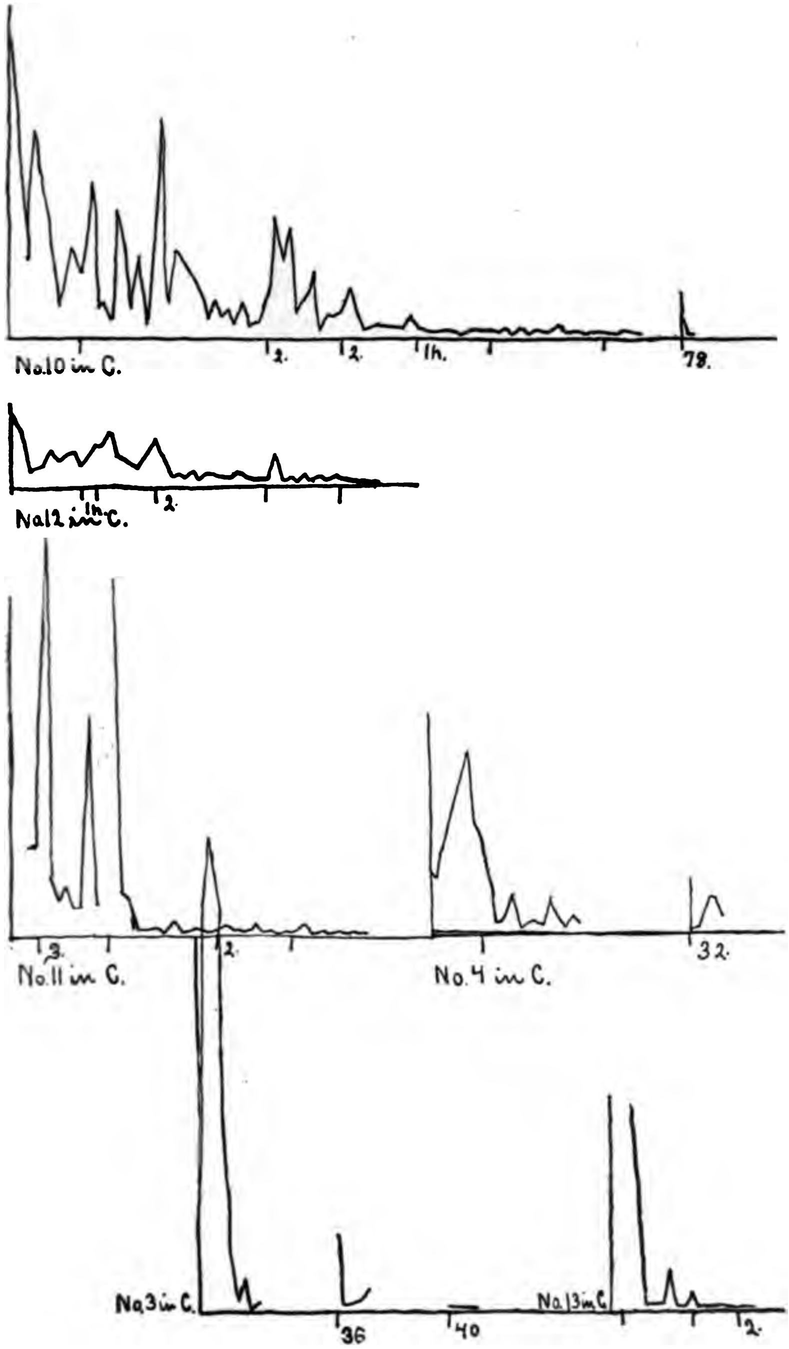

No.10 in C.
2.
2.
1h.
78.
No.12 in C.
1h.
2.
3.
2.
No.11 in C.
No.4 in C.
32.
No.3 in C.
36
40
No.13 in C.
2.

Fig. 3.

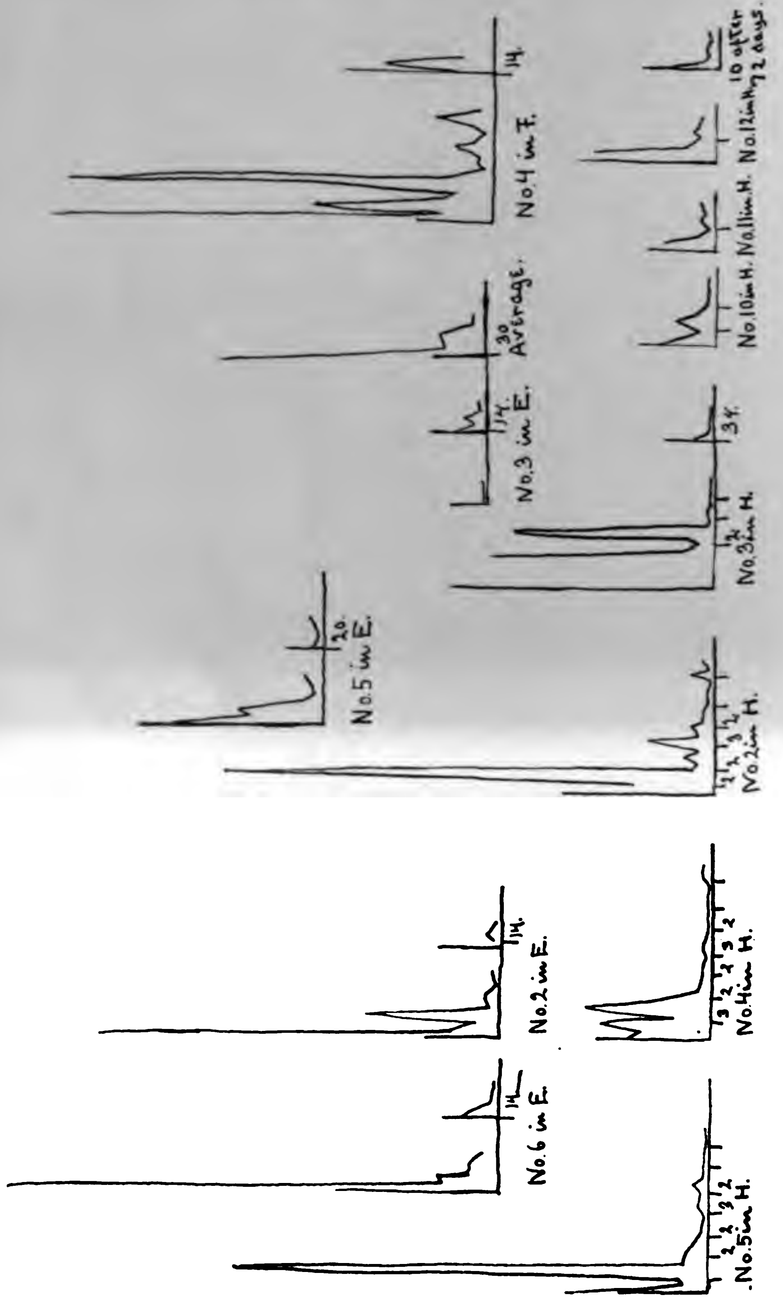

No.5 in E.
20.
No.2 in H.
No.3 in H.
34.
No.3 in E.
14.
30
Average.
No.4 in F.
14.
No.10 in H.
No.11 in H.
No.12 in H.
10 after 72 days.
No.6 in E.
14.
No.2 in E.
14.
No.5 in H.
No.4 in H.

FIG. 4.

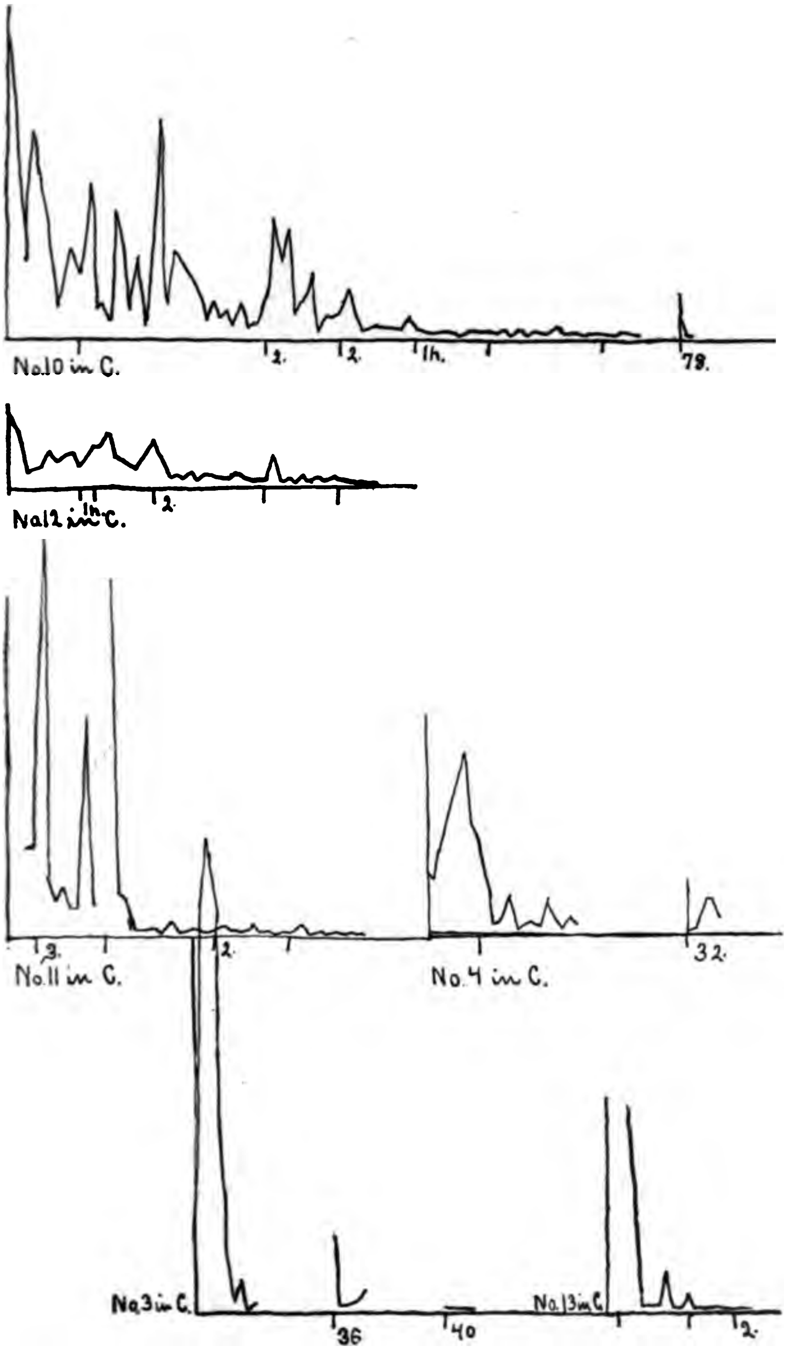

No.10 in C.
2.
2.
1h.
78.
No.12 in C.
1h.
2.
No.11 in C.
3.
2.
No.4 in C.
32.
No.3 in C.
36
40
No.13 in C.
2.

FIG. 3.

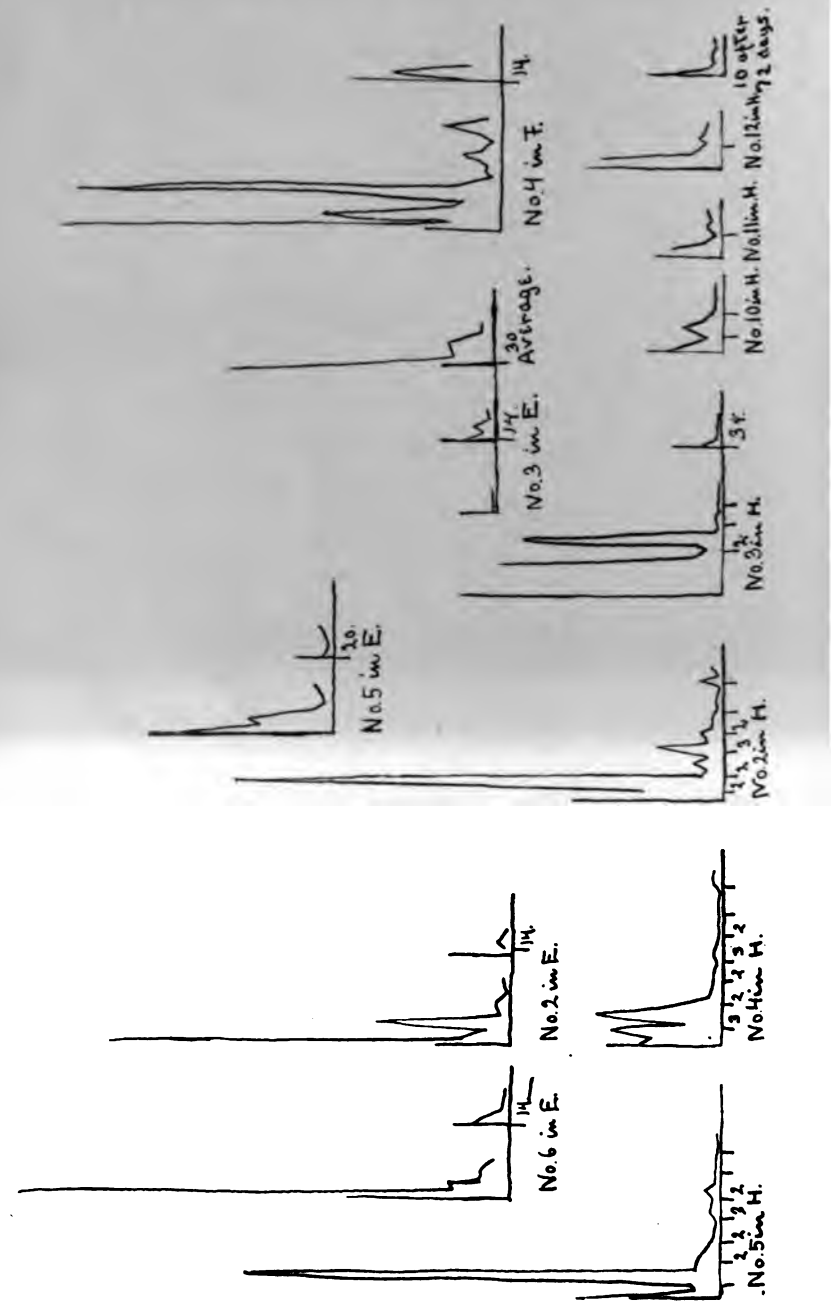

No.4 in F.
14.
No.3 in E.
14.
30
Average.
No.5 in E.
20.
No.2 in H.
No.3 in H.
34.
No.10 in H.
No.11 in H.
No.12 in H.
10 after 72 days.
No.6 in E.
14.
No.2 in E.
14.
No.5 in H.
No.4 in H.

FIG. 4.

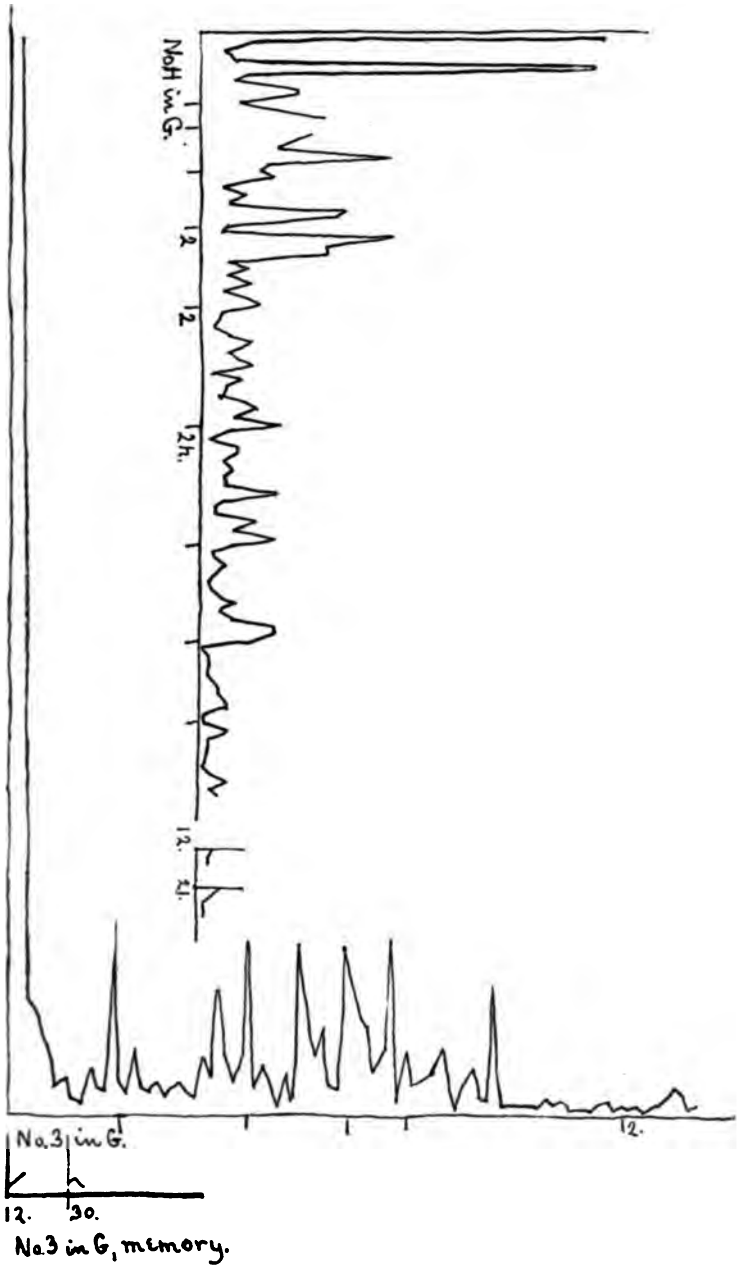


FIG. 5.

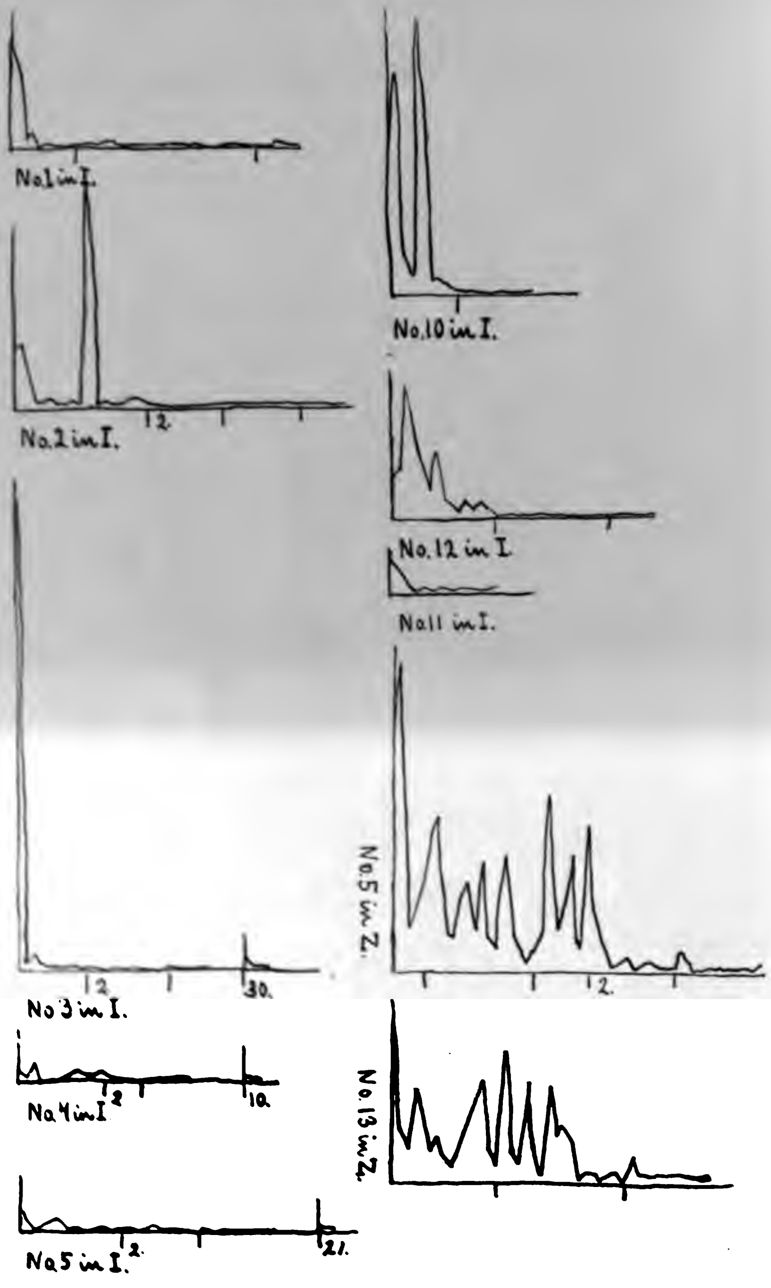

No.1 in I.
No.2 in I.
12.
No.3 in I.
12
30.
No.4 in I.
2
10.
No.5 in I.
2.
21.
No.10 in I.
No.12 in I.
No.11 in I.
No.5 in Z.
2.
No.13 in Z.

FIG 6

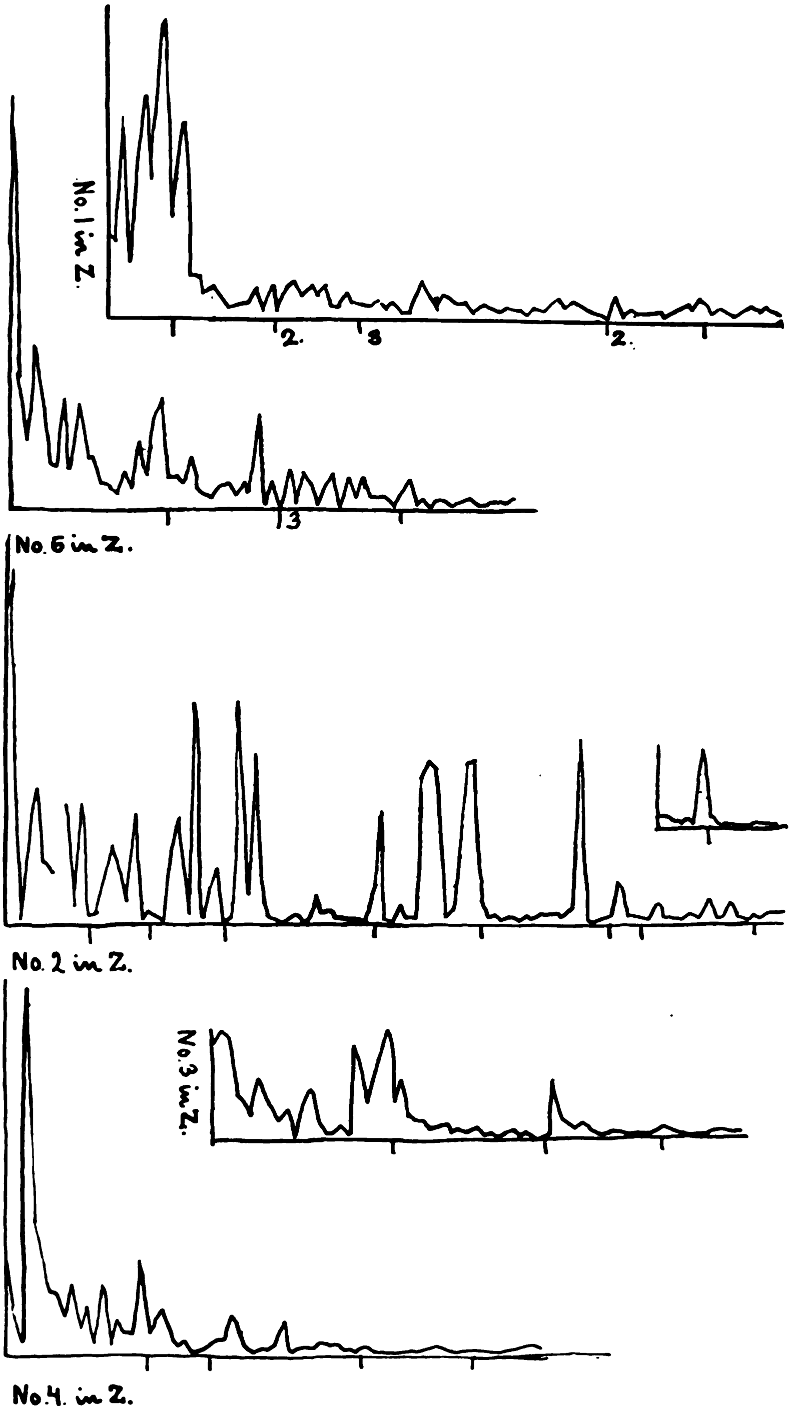


FIG. 7.

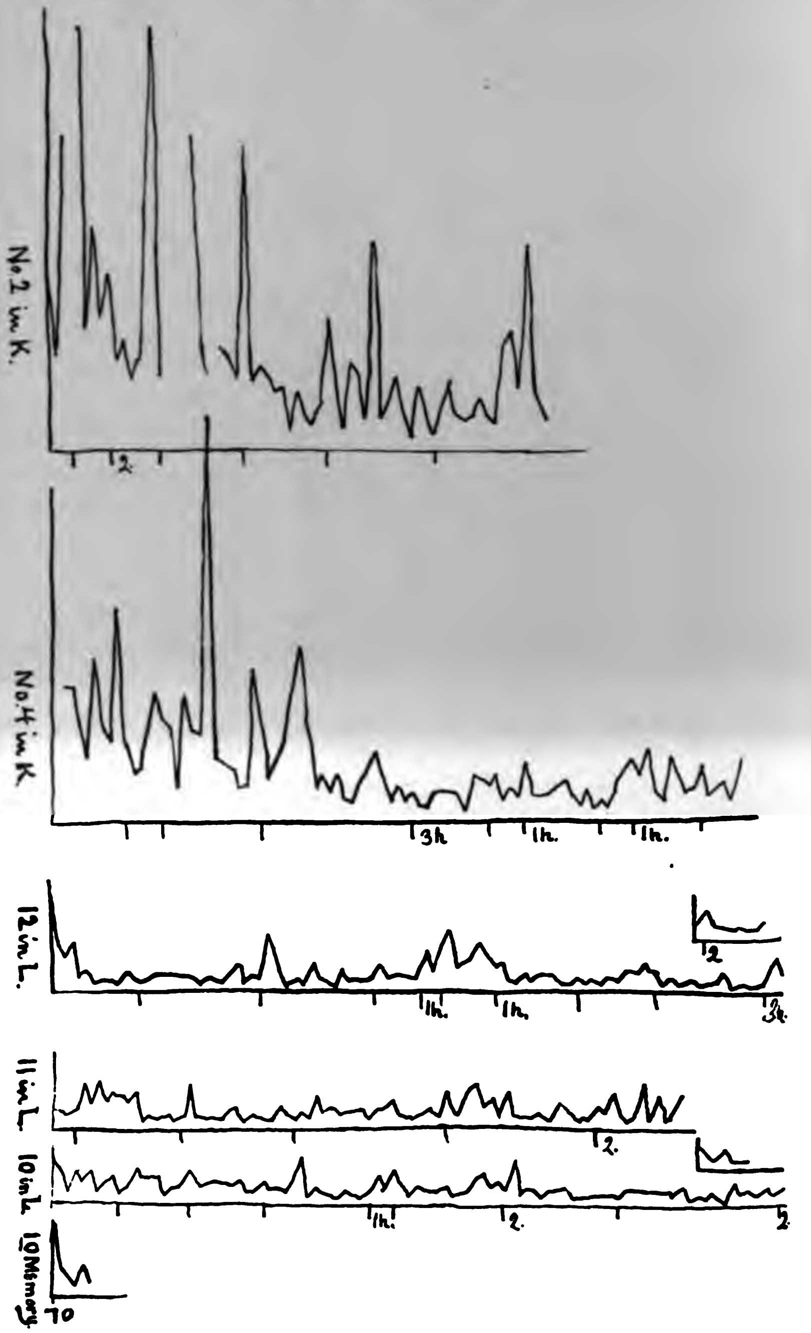

No. 2 in K.
2
No. 4 in K.
3h
1h.
1h.
12 in L.
2
1h.
1h.
3h.
11 in L.
2.
10 in L.
1h.
2.
2.
10 Memory
70

FIG. 10.

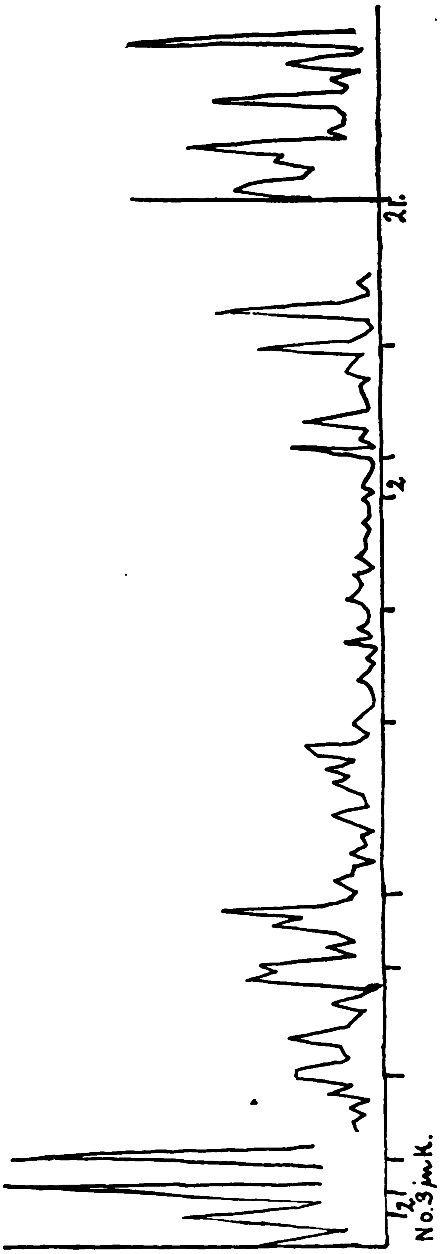


FIG. 9.

thes cases a noticeable tendency, of the cause of which m norant, to diminish the act until it becomes a mere ves- e a lick or scratch. After the cat gets so that it performs the act soon after being put in, it begins to do it less and less vigorously. The licking degenerates into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch, the cat waves its paw up and down rapidly for an instant. Moreover, if sometimes you do not let the cat out after this feeble reaction, it does not at once repeat the movement, as it would do if it depressed a thumb-piece, for instance, without success in getting the door open. Of the reason for this difference I am again ignorant.

Previous experience makes a difference in the quickness with which the cat forms the associations. After getting out of six or eight boxes by different sorts of acts the cat's general tendency to claw at loose objects within the box is strengthened and its tendency to squeeze through holes and bite bars is weakened; accordingly it will learn associations along the general line of the old more quickly. Further, its tendency to pay attention to what it is doing gets strengthened, and this is something which may properly be called a change in degree of intelligence. A test was made of the influence of experience in this latter way by putting two groups of cats through I (lever), one group (1, 2, 3, 4, 5) after considerable experience, the other (10, 11, 12) after experience with only one box. As the act in I was not along the line of the acts in previous boxes, and as a decrease in the squeezings and bitings would be of little use in the box as arranged, the influence of experience in the former way was of little account. The curves of all are shown on page 22.

If the whole set of curves are examined in connection with the following table, which gives the general order in which each animal took up the different associations which he eventually formed, many suggestions of the influence of experience will be met with. The results are not exhaustive enough to justify more than the general conclusion that there is such an influence. By taking more individuals and thus eliminating all other factors besides experience, one can easily show just how and how far experience facilitates association.

When in this table the letters designating the boxes are in italics it means that, though the cat formed the association, it was in connection with other experiments and so is not recorded in the curves.

Cat 1	*A* *B* *C* D_1 *D* Z I.
" 2	*C* D_1 *D* E Z H J I K.
" 3	A C E G H J Z I K.
" 4	C F G D Z H J I K.
" 5	*C* E Z H I.
" 6	*A* *C* E Z.
" 7	*A* *C*
" 10	C I A H D L
" 11	C I A H D L
" 12	C I A H D L
" 13	A C D G Z

The advantage due to experience in our experiments is not, however, the same as ordinarily in the case of trained animals. With them the associations are with the acts or voice of man or with sense impressions to which they naturally do not attend (*e.g.*, figures on a blackboard, ringing of a bell, some act of another animal). Here the advantage of experience is mainly due to the fact that by such experience the animals gain the habit of attending to the master's face and voice and acts and to sense impressions in general.

I made no attempt to find the differences in ability to acquire associations due to age or sex or fatigue or circumstances of any sort. By simply finding the average slope in the different cases to be compared, one can easily demonstrate any such differences that exist. So far as this discovery is profitable, investigation along this line ought now to go on without delay, the method being made clear. Of differences due to difference in the species, genus, etc. Of the animals I will speak after reviewing the time-curves of dogs and chicks.

In the present state of animal psychology there is another value to these results which was especially aimed at by the investigator from the start. They furnish a quantitative estimate of what the average cat can do, so that if any one has an animal which he thinks has shown superior intelligence or perhaps reasoning power, he may test his observations and opinion by taking the time-curves of the animal in such boxes as I have described.[1]

If his animal in a number of cases forms the associations very much more quickly, or deals with the situation in a more intelligent fashion than my cats did, then he may have ground for claiming in his individual a variation toward greater intelligence and possibly intelligence of a different order. On the other hand, if the animal fails to rise above the type in his dealings with the boxes, the observer should confess that his opinion of the animal's intelligence may have been at fault and should look for a correction of it.

We have in these time-curves a fairly adequate measure of what the ordinary cat can do, and how it does it, and in similar curves soon to be presented a less adequate measure of what a dog may do. If other investigators, if especially all amateurs who are interested in animal intelligence, will take other cats and dogs, especially those supposed by owners to be extraordinarily intelligent, and experiment with them in this way, we shall soon get a notion of how much variation there is among animals in the direction of more or superior intelligence. The beginning here made is meager but solid. The knowledge it gives needs to be much extended. The variations found in individuals should be correlated not merely with supposed superiority in intelligence, a factor too vague to be very serviceable, but with observed differences in vigor, attention, memory and muscular skill. No phenomena are more capable of exact and thorough investigation by experiment than the associations of animal consciousness. Never will you get a better psychological subject than a hungry cat. When the crude beginnings of this research have been improved and replaced by more ingenious and adroit experimenters, the results ought to be very valuable.

---

[1] To any such person who may chance to read this monograph I may say that I will gladly furnish him photographs of the boxes, so that he may work with exact duplicates of them.

Surely every one must agree that no man now has a right to advance theories about what is in animals' minds or to deny previous theories unless he supports his thesis by systematic and extended experiments. My own theories, soon to be proclaimed, will doubtless be opposed by many. I sincerely hope they will, provided the denial is accompanied by actual experimental work. In fact I shall be tempted again and again in the course of this book to defend some theory, dubious enough to my own mind, in the hope of thereby inducing some one to oppose me and in opposing me to make the experiments I have myself had no opportunity to make yet. Probably there will be enough opposition if I confine myself to the theories I feel sure of.

EXPERIMENTS WITH DOGS.

The boxes used were as follows:

AA was similar to A (O at front), except that the loop was of stiff cord ⅜ inch in diameter and was larger (3½ inches diameter); also it was hung a foot from the floor and 8 inches to the right of the door. The box itself was 41 x 20 x 23.

BB was similar to B, the loop being the same as in AA, and being hung a foot from the floor. The box was of the same size and shape as AA.

BB1 was like BB, but the loop was hung 18 inches from the floor.

CC was similar to C (Button), but the button was 6 inches long, and the box was 36½ x 22 x 23.

II was similar to I, but the box was 30 x 20 x 25 inches; the door (11 inches wide, 6 high) was in the left front corner, and the lever was 6 inches long and entered the box at a point 2 inches to the right of the door and 4 inches above the floor.

In M the same box as in II was used, but instead of a lever projecting inside the box, a lever running outside parallel to the plane of the front of the box and 18 inches long was used. This lay close against the bars composing the front of the box, and could be pawed down by sticking the paw out an inch or so between two bars, at a point about 15 inches high and 6 inches in from the right edge of the front. We may call M '*Lever outside.*'

N was a pen 5 × 3 feet made of wire netting 46 inches high. The door, 31 × 20, was in the right half of the front. A string from the bolt passed up over a pulley and back to the back center, where it was fastened 33 inches above the floor. Biting or pawing this string opened the door.

O was like K, except that there was only one bar, that the string ran inside the box, so that it was easily accessible, and that the bolt raised in K by depression of the platform could be raised in O (and was by the dog experimented on) by sticking the muzzle out between two bars just above the bolt and by biting the string, at the same time jerking it upward. O was 30 × 20 × 25 in size.

The box G was used for both dogs and cats, without any variation save that for dogs the resistance of the door to pressure outwards was doubled.

In these boxes were put in the course of the experiments dog 1 (about 8 months old), and dogs 2 and 3, adults, all of small size.

A dog who, when hungry, is shut up in one of these boxes is not nearly so vigorous in his struggles to get out as is the young cat. And even after he has experienced the pleasure of eating on escape many times he does not try to get out so hard as a cat, young or old. He does try to a certain extent. He paws or bites the bars or screening, and tries to squeeze out in a tame sort of way. He gives up his attempts sooner than the cat, if they prove unsuccessful. Furthermore his attention is taken by the food, not the confinement. He wants to get *to* the food, not *out of* the box. So, unlike the cat, he confines his efforts to the front of the box. It was also a practical necessity that the dogs should be kept from howling in the evening, and for this reason I could not use as a motive the utter hunger which the cats were made to suffer. In the morning, when the experiments were made, the dogs were surely hungry, and no experiment is recorded in which the dog was not in a state to be willing to make a great effort for a bit of neat, but the motive may not have been even and equal hroughout, as it was with the cats.

The curves which follow are to be interpreted in the same vay as those for the cats, and are on the same scale. The rder in which No. 1 took up the various associations was AA, B, BB1, G, N, CC, II, O.

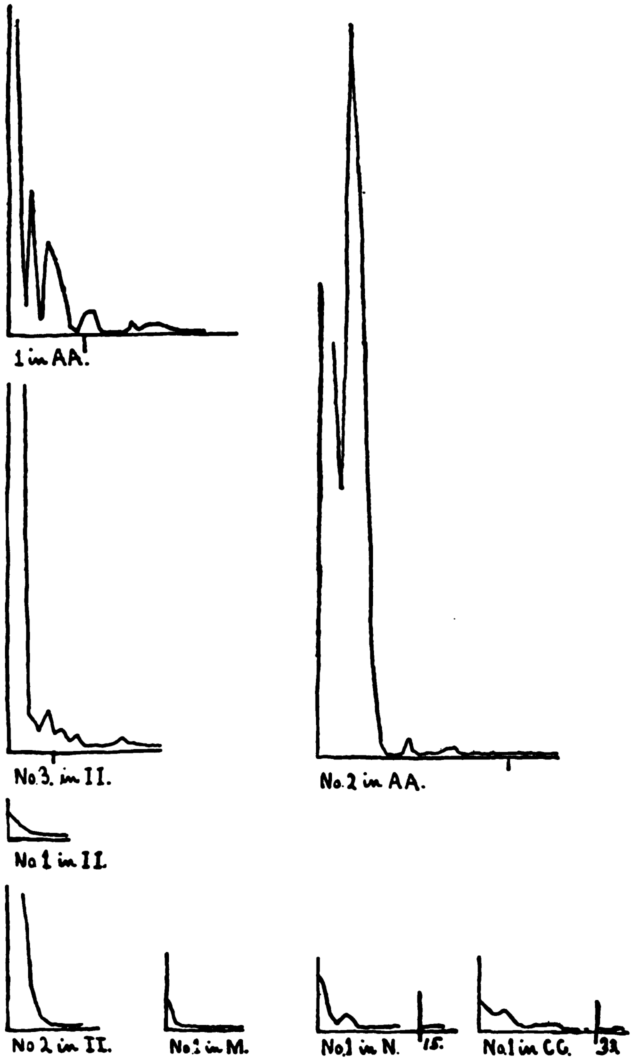


FIG. 11.

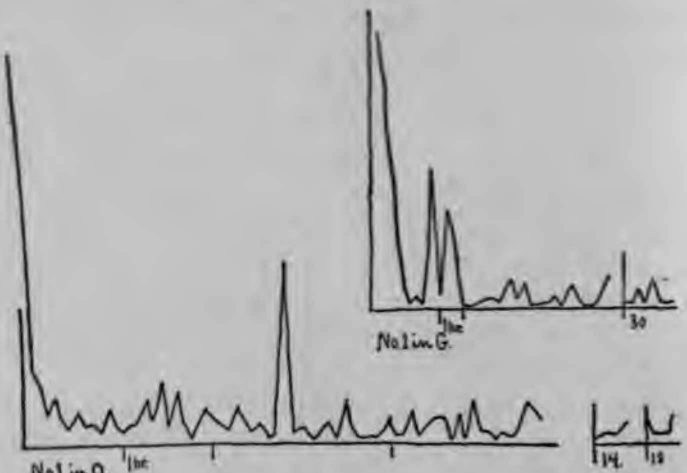


FIG. 12.

The percentage of dogs succeeding in the various boxes is given below, but is of no consequence, because so few were tried, and because the motive, hunger, was not perhaps strong enough, or equal in all cases.

In AA 3 out of 3.
" BB 0 " " 2 (that is, without previous experience of AA).
In CC 1 out of 2.
" II 3 " " 3.
" M 1 " " 2.
" N 1 " " 3.
" G 1 " " 3.

EXPERIMENTS WITH CHICKS.

The apparatus was as follows:

P was simply a small pen arranged with two exits, one leading to the enclosure where were the other chicks and food, one leading to another pen with no exit. The drawing (Fig. 13 on page 35) explains itself. A chick was placed at A and left to find its way out. The walls were made of books stuck up on end.

Q was a similar pen arranged so that the real exit was harder to find. (See Fig. 14.)

R was still another pen similarly constructed, with four possible avenues to be taken. (See Fig. 15.)

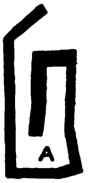


FIG. 13.

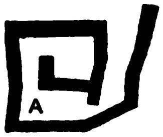


FIG. 14.

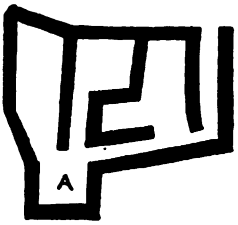


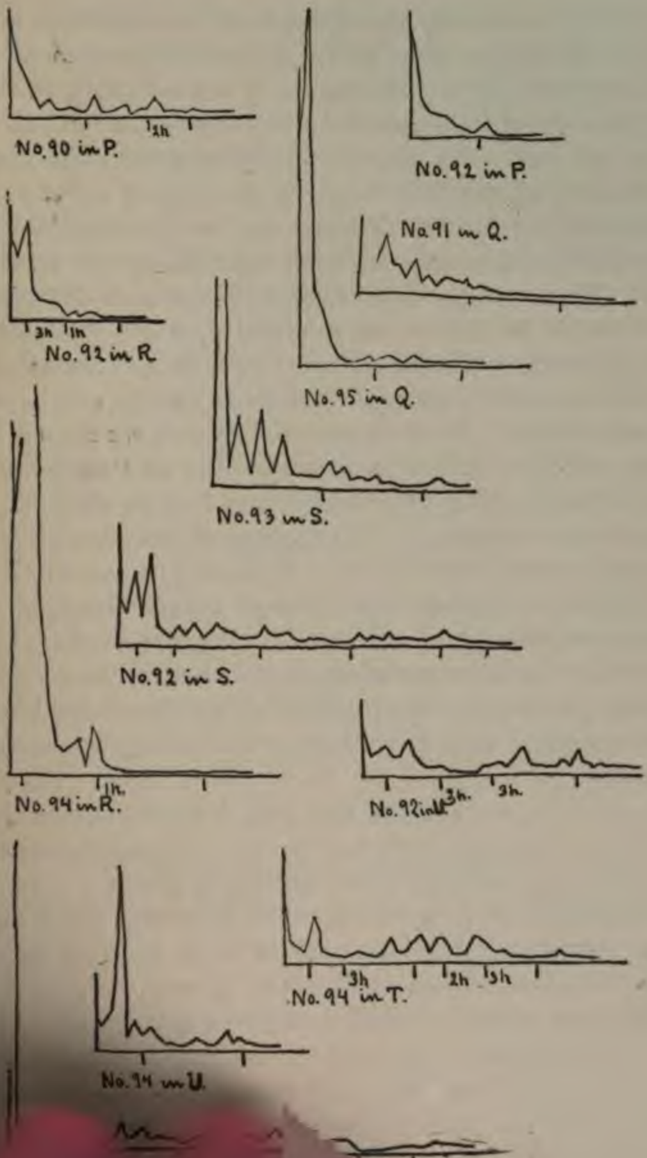
FIG. 15.

S was a pen with walls 11 inches high. On the right side an inclined plane of wire screening led from the floor of the pen to the top of its front wall. Thence the chick could jump down to where its fellows and the food and drink were. S was 17 x 14 in size.

T was a pen of the same size as S, with a block of wood 3 inches by 3 and 2 inches high in the right back corner. From this an inclined plane led to the top of the front wall (on the right side of the box). But a partition was placed along the left edge of this plane, so that a chick could reach it only via the wooden block, not by a direct jump.

U was a pen 16 x 14 x 10 inches. Along the back toward the right corner were placed a series of steps 1½ inches wide, the first 1, the second 2, and the third 3 inches high. In the corner was a platform 4 x 4, and 4 high, from which access to the top of the front wall of the pen could be gained by scrambling up inside a stovepipe 11 inches long, inclined upward at an angle of about 30°. From the edge of the wall the chick could of course jump down to food and society. The top of the pen was covered so that the chick could not from the platform jump onto the edge of the stovepipe or the top of the pen wall. The only means of exit was to go up the steps to the platform, up through the stovepipe to the front wall, and then jump down.

The time-curves for chicks 90, 91, 92, 93, 94 and 95, all 2–8 days old when experimented on, follow on page 37. The scale is the same as that in the curves of the cats and dogs.



r that such differences
chicks are surely very

Besides these simple acts, which any average chick will accidentally hit upon and associate, there are, in the records of my preliminary study of animal intelligence, a multitude of all sorts of associations which some chicks have happened to form. Chicks have escaped from confinement by stepping on a little platform in the back of the box, by jumping up and pulling a string like that in D, by pecking at a door, by climbing up a spiral staircase and out through a hole in the wall, by doing this and then in addition walking across a ladder for a foot to another wall from which they could jump down, etc., etc. Not every chick will happen upon the right way in these cases, but the chicks who did happen upon it all formed the associations perfectly after enough trials.

The behavior of the chicks shows the same general character as that of the cat, conditioned, of course, by the different nature of the instinctive impulses. Take a chick put in T (inclined plane) for an example. When taken from the food and er chicks and dropped into the pen he shows evident signs discomfort; he runs back and forth, peeping loudly, trying squeeze through any openings there may be, jumping up to get over the wall, and pecking at the bars or screen, if such separate him from the other chicks. Finally, in his general running around he goes up the inclined plane a way. He may come down again, or he may go on up far enough to see over the top of the wall. If he does, he will probably go running up the rest of the way and jump down. With further trials he gains more and more of an impulse to walk up an inclined plane when he sees it, while the vain running and pecking, etc., are stamped out by the absence of any sequent pleasure. Finally, the chick goes up the plane as soon as put in. In scientific terms this history means that the chick, when confronted by loneliness and confining walls, responds by those acts which in similar conditions in nature would be likely to free him. Some one of these acts leads him to the successful act, and the resulting pleasure stamps it in. Absence of pleasure stamps all others out. The case is just the same as with dogs and cats. The time-curves are shown in Fig. 16.

Coming now to the question of differences in intelligence

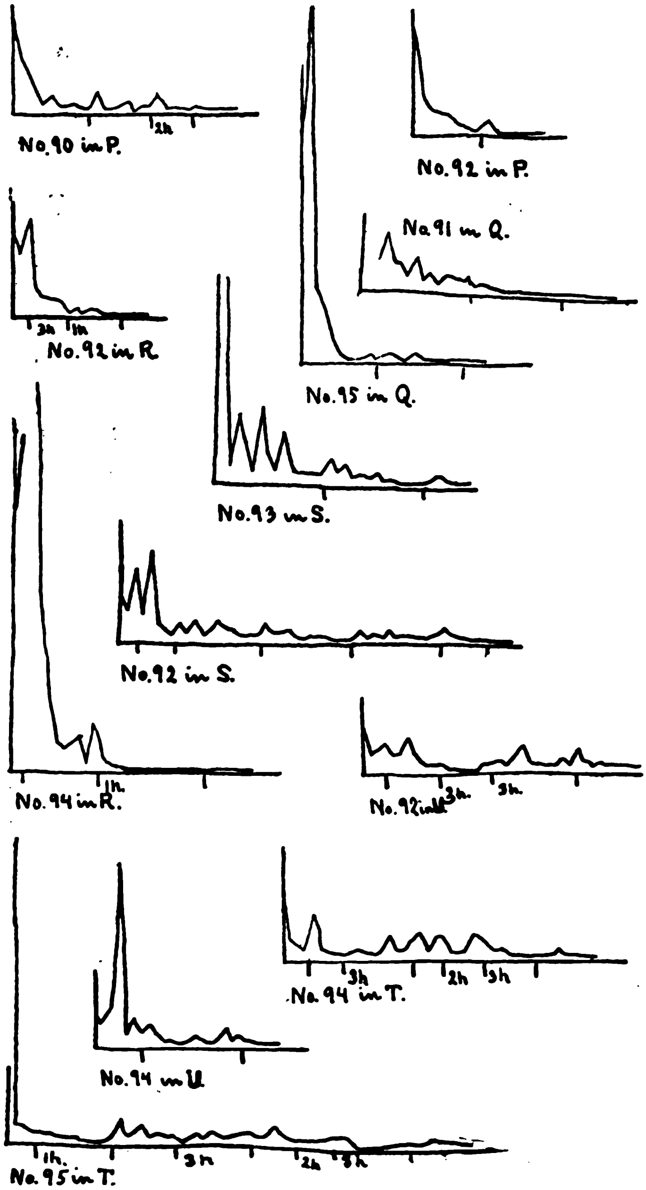


FIG. 16.

etween the different animals, it is clear that such differences re hard to estimate accurately. The chicks are surely very much slower in forming associations and less able to tackle hard ones, but the biggest part of the difference between what they do and what the dogs and cats do is not referable so much to any difference in intelligence as to a difference in their bodily organs and instinctive impulses. As between dogs and cats, the influence of the difference in quantity of activity, in the direction of the instinctive impulses, in the versatility of the fore-limb, is hard to separate from the influence of intelligence proper. The best practical tests to judge such differences in general would be differences in memory, which are very easily got at, differences in the delicacy and complexity attainable, and of course differences in the slope of the curves for the same association. If all these tests agreed, we should have a right to rank one animal above the other in a scale of intelligence. But this whole question of grading is, after all, not so important for comparative psychology as its popularity would lead one to think. Comparative psychology wants first of all to trace human intellection back through the phylum to its origin, and in this aim is helped little by knowing that dogs are brighter than cats, or whales than seals, or horses than cows. Further, the whole question of 'intelligence' should be resolved into particular inquiries into the development of attention, activity, memory, etc.

So far as concerns dogs and cats, I should decide that the former were more generally intelligent. The main reason, however, why dogs seem to us so intelligent is not a good reason for the belief. It is because, more than any other domestic animal, they direct their attention to *us*, to what we do, and so form associations connected with acts of ours.

Having finished our attempt to give a true description of the facts of association, so far as observed from the outside, we may now progress to discuss its inner nature. A little preface about certain verbal usages is necessary before doing so. Throughout I shall use the word 'animal' or 'animals,' and the reader might fancy that I took it for granted that the associative processes were the same in all animals as in these cats and dogs of mine. Really, I claim for my animal psychology only that it is the psychology of just these particular animals. What this warrants about animals in general may be left largely to the discretion of the reader. As I shall later say, it is probable that in regard to imitation and the power of forming associations from a lot of free ideas, the anthropoid primates are essentially different from the cats and dogs.

The reasons why I say 'animals' instead of 'dogs and cats of certain ages' are two. I do think that the probability that the other mammals, barring the primates, offer no objections to the theories here advanced about dogs and cats, is a very strong probability, strong enough to force the burden of proof upon any one who should, for instance, say that horse-goat psychology was not like cat-dog psychology in these general matters. I should claim that, till the contrary was shown in any case, my statements should stand for the mammalian mind in general, barring the primates. My second reason is that I hate to burden the reader with the disgusting rhetoric which would result if I had to insert particularizations and reservations at every step. The word 'animal' is too useful, rhetorically, to be sacrificed. Finally, inasmuch as most of my theorizing will be in the line of denying certain relatively high functions to animals, the evidence from cats and dogs is sufficient, for they are from among the most intelligent animals, and functions of the kind to be discussed, if absent in their case, are probably absent from the others.

Reasoning or Inference.

The first great question is whether or not animals are ever led to do any of their acts by reasoning. Do they ever conclude from inference that a certain act will produce a certain desired result, and so do it? The best opinion has been that they do not. The best interpretation of even the most extraordinary performances of animals has been that they were the result of accident and association or imitation. But it has after all been only opinion and interpretation, and the opposite theory persistently reappears in the literature of the subject. So, although it is in a way superfluous to give the *coup de grâce* to the despised theory that animals reason, I think it is worth while to settle this question once for all.

The great support of those who do claim for animals the ability to infer has been their wonderful performances which resemble our own. These could not, they claim, have happened by accident. No animal could learn to open a latched gate by accident. The whole substance of the argument vanishes if, as a matter of fact, animals do learn those things by accident. *They certainly do.* In this investigation choice was made of the intelligent performances described by Romanes in the following passages. I shall quote at some length because these passages give an admirable illustration of an attitude of investigation which this research will, I hope, render impossible for any scientist in the future. Speaking of the general intelligence of cats, Romanes says:

"Thus, for instance, while I have only heard of one solitary case * * * of a dog which, without tuition, divined the use of a thumb-latch so as to open a closed door by jumping on the handle and depressing the thumb-piece, I have received some half-dozen instances of this display of intelligence on the part of cats. These instances are all such precise repetitions of one another that I conclude the fact to be one of tolerably ordinary occurrence among cats, while it is certainly very rare among dogs. I may add that my own coachman once had a cat which, certainly without tuition, learnt thus to open a door that led into the stables from a yard into which looked some of the windows of the house. Standing at these windows when the cat did not see me, I have many times witnessed her *modus operandi.* Walking up to the door with a most matter-of-course kind of air, she used to spring at the half hoop handle just below the thumb-latch. Holding on to the bottom of this half-hoop with one fore paw, she then raised the other to the thumb-piece, and while depressing the latter finally with her hind legs scratched and pushed the door-posts so as to open the door * * *.

"Of course in all such cases the cats must have previously observed that the doors are opened by persons placing their hands upon the handles and, having observed this, the animals act by what may be strictly termed rational imitation. But it should be observed that the process as a whole is something more than imitative. For not only would observation alone be scarcely enough (within any limits of thoughtful reflection that it would be reasonable to ascribe to an animal) to enable a cat upon the ground to distinguish that the essential part of the process consists not in grasping the handle, but in depressing the latch; but the cat certainly never saw any one, after having depressed the latch, pushing the door-posts with his legs; and that this pushing action is due to an originally deliberate intention of opening the door, and not to having accidentally found this action to assist the process, is shown by one of the cases communicated to me; for in this case, my correspondent says, 'the door was not a loose-fitting one, by any means, and I was surprised that by the force of one hind leg she should have been able to push it open after unlatching it.' Hence we can only conclude that the cats in such cases have a very definite idea as to the mechanical properties of a door: they know that to make it open, even when unlatched, it requires to be *pushed*—a very different thing from trying to imitate any particular action which they may see to be performed for the same purpose by man. The whole psychological process, therefore, implied by the fact of a cat opening a door in this way is really most complex. First the animal must have observed that the door is opened by the hand grasping the handle and moving the latch. Next she must reason, by 'the logic of feelings'—'If a hand can do it, why not a paw?' Then strongly moved by this idea she makes the first trial. The steps which follow have not been observed, so we cannot certainly say whether she learns by a succession of trials that depression of the thumb-piece constitutes the essential part of the process, or, perhaps more probably, that her initial observations supplied her with the idea of clicking the thumb-piece. But, however this may be, it is certain that the pushing with the hind feet after depressing the latch must be due to adaptive reasoning unassisted by observation; and only by the concerted action of all her limbs in the performance of a highly complex and most unnatural movement is her final purpose attained." (Animal Intelligence, pp. 420–422.)

A page or two later we find a less ponderous account of a cat's success in turning aside a button and so opening a window:

"At Parara, the residence of Parker Bowman, Esq., a full-grown cat was one day accidentally locked up in a room without any other outlet than a small window, moving on hinges, and kept shut by means of a swivel. Not long afterwards the window was found open and the cat gone. This having happened several times, it was at last found that the cat jumped upon the window-sill, placed her fore-paws as high as she could reach against the side, deliberately reached with one over to the swivel, moved it from its horizontal to a vertical position, and then, leaning with her whole weight against the window, swung it open and escaped." (Animal Intelligence, p. 425.)

A description has already been given on page 9 of the small box (C), whose door fell open when that button was turned, and also of a large box (CC) for the dogs, with a similar door. The thumb-latch experiment was carried on with the same box (G) for both cats and dogs, but the door was arranged so that a greater force (1.3 kilograms) was required in the case of the dogs. It will be remembered that the latch was so fixed that if the thumb-piece were pressed down, without contemporaneous outward pressure of the door, the latch bar would merely drop back into its catch as soon as the paw was taken off the door. If, however, the door were pushed outward the latch bar, being pressed closely against the outer edge of its catch, would, if lifted, be likely to fall outside it and so permit the door to open if then or later sufficient pressure were exerted. Eight cats (Nos. 1, 2, 3, 4, 5, 6, 7 and 13) were, one at a time, left in this thumb-latch box. All exhibited the customary instinctive clawings and squeezings and bitings. Out of the eight all succeeded in the course of their vigorous struggles in pressing down the thumb-piece, so that if the door had been free to swing open they could have escaped. Six succeeded in pushing both thumb-piece down and door out, so that the bar did not fall back into its place. Of these five succeeded in also later pushing the door open, so that they escaped and got the fish outside. Of these, three, after repeated trials, associated the complicated movements required with the sight of the interior of the box so firmly that they attacked the thumb-latch the moment they were put in. The history of the formation of the association in the case of 3 and of 4 is shown in the curves on page 21. In the case of 13 the exact times were not taken. The combination of accidents required was enough to make it take No. 1 and No. 6 a long time to get out. Consequently, weariness and failure inhibited their impulses to claw, climb, etc., more than the rare pleasure from getting out strengthened them, and they failed to form the association. Like the cats who utterly failed to get out, they finally ceased to try when put in. The history of their efforts is as follows: The figures in the columns represent the time (in minutes and seconds) the animal was in the box before escaping or before being taken out if he failed to escape. Cases of failure are designated by an F after the figures. Double lines represent an interval of twenty-four hours.

No. 1.	No. 6.
13.00 F	17.50
9.30	3.30
1.40	9.00
.50	2.10
15.00	1.45
6.00 F	1.55
14.00	13.00
20.00 F	5.00
4.30	2.30
20.00 F	15.00
20.00 F	10.00 F
15.00 F	5.00
60.00 F	15.00 F
	10.00 F
	10.00 F

It should be noted that, although cats 3 and 4 had had some experience in getting out of boxes by clawing at loops and turning buttons, they had never had anything at all like a thumb-latch to claw at, nor had they ever seen the door opened by its use, nor did they even have any experience of the fact that the part of the box where the thumb-piece was was the door. And we may insert here, what will be stated more fully later, that there was displayed no observation of the surroundings or deliberation upon them. It was just a mad scramble to get out.

Three dogs (1, 2 and 3) were given a chance to liberate themselves from this same box. 2 and 3, who were rather inactive, failed to even push the thumb-piece down. No. 1, who was very active, did push it down at the same time that she happened to be pushing against the door. She repeated this and formed the association as shown in the curve on page 34. She had had experience only of escaping by pulling a loop of string.

Out of 6 cats who were put in the box whose door opened by a button, not one failed, in the course of its impulsive activity, to push the button around. Sometimes it was clawed one side from below; sometimes vigorous pressure on the top turned it around; sometimes it was pushed up by the nose. No cat who was given repeated trials failed to form a perfect association between the sight of the interior of that box and the proper movements. Some of these cats had been in other boxes where pulling a loop of string liberated them, 3 and 4 had had considerable experience with the boxes and probably had acquired a general tendency to claw at loose objects. 10, 11 and 12 had never been in *any box* before. The curves are on page 19.

Of two dogs, one, when placed in a similar but larger box, succeeded in hitting the button in such a way as to let the door open, and formed a permanent association, as shown by the curves on page 33. No one who had seen the behavior of these animals when trying to escape could doubt that their actions were directed by instinctive impulses, not by rational observation. It is then absolutely sure that a dog or cat *can* open a door closed by a thumb-latch or button, merely by the accidental success of its natural impulses. If *all* cats, when hungry and in a *small* box, will accidentally push the button that holds the door, an *occasional* cat in a *large* room may very well do the same. If three cats out of eight will accidentally press down a thumb-piece and push open a small door, three cats out of a thousand may very well open doors or gates in the same way.

But besides thus depriving of their value the facts which these theorizers offer as evidence, we may, by a careful examination of the method of formation of these associations as it is shown in the time-curves, gain positive evidence that no power of inference was present in the subjects of the experiments. Surely if 1 and 6 had possessed any power of inference they would not have failed to get out after having done so several times. Yet they did (see p. 43). If they had once even, much less if they had six or eight times, inferred what was to be done, they should have made the inference the seventh or ninth time. And if there were in these animals any power of inference, however rudimentary, however sporadic, however dim, there should have appeared among the multitude some cases where an animal, seeing through the situation, knows the proper act, does it, and from then on does it immediately upon being confronted with the situation. There ought, that is, to be a sudden vertical descent in the time-curve. Of course, where the act resulting from the impulse is very simple, very obvious, and very clearly defined, a simple experience may make the association perfect, and we may have an abrupt descent in the time-curve without needing to suppose inference. But if in a complex act, a series of acts or an ill-defined act, one found such a sudden consummation in the associative process, one might very well claim that reason was at work. Now, the scores of cases recorded show no such phenomena. The cat does not look over the situation, much less *think* it over, and then decide what to do. It bursts out at once into the activities which instinct and experience have settled on as suitable reactions to the situation '*confinement when hungry with food outside.*' It does not ever in the course of its successes realize that such an act brings food and therefore decide to do it and thenceforth do it immediately from *decision* instead of from impulse. The one impulse, out of many accidental ones, which leads to pleasure, becomes strengthened and stamped in thereby, and more and more firmly associated with the sense-impression of that box's interior. Accordingly it is sooner and sooner fulfilled. Futile impulses are gradually stamped out. The gradual slope of the time-curve, then, shows the absence of reasoning. They represent the wearing smooth of a path in the brain, not the decisions of a rational consciousness.

In a later discussion of imitation further evidence that animals do not reason will appear. For the present, suffice it to say, that a dog, or cat, or chick, who does not in his own impulsive activity learn to escape from a box by pulling the proper loop, or stepping on a platform, or pecking at a door, will not learn it from seeing his fellows do so. They are incapable of even the inference (if the process may be dignified by that name) that what gives another food will give it to them also. So, also, it will be later seen that an animal cannot learn an act by being put through it. For instance, a cat who fails to push down a thumb-piece and push out the door cannot be taught by having one take its paw and press the thumb-piece down with it. This *could* be learned by a certain type of associative process without inference. *Were there inference it surely would be learned.*

Finally, attention may be called to the curves which show the way that the animal mind deals with a series of acts (*e. g.*, curves for G, J, K, L and O, found on pages 21 to 26 and 34). Were there any reasoning the animals ought early to master the method of escape in these cases (see descriptions on pages 10 to 12 and 32) so as to do the several acts in order, and not to repeat one after doing it once, or else ought utterly to fail to master the thing. But, in all these experiments, where there was every motive for the use of any reasoning faculty, if such existed, where the animals literally lived by their intellectual powers, one finds no sign of abstraction, or inference, or judgment.

So far I have only given facts which are quite uninflenced by any possible incompetence or prejudice of the observer. These alone seem to disprove the existence of any rational faculty in the subjects experimented on. I may add that my observations of all the conduct of all these animals during the months spent with them, failed to find any act that even *seemed* due to reasoning. I should claim that this quarrel ought now to be dropped for good and all, that investigation ought to be directed along more sensible and profitable lines. I should claim that the psychologist who studies dogs and cats in order to defend this 'reason' theory is on a level with a zoölogist who should study fishes with a view to supporting the thesis that they possessed clawed digits. The rest of this account will deal with more promising problems, of which the first, and not the least important, concerns the facts and theories of *imitation.*

IMITATION.

To the question, 'Do animals imitate?' science has uniformly answered, 'Yes.' But so long as the question is left in this general form, no correct answer to it is possible. It will be seen, from the results of numerous experiments soon to be described, that imitation of a certain sort is not possible for animals, and before entering upon that description it will be helpful to differentiate this matter of imitation into several varieties or aspects. The presence of some sorts of imitation does not imply that of other sorts.

There are, to begin with, the well-known phenomena presented by the imitative birds. The power is extended widely, ranging from the parrot who knows a hundred or more articulate sounds to the sparrow whom a patient shoemaker taught to get through a tune. Now, if a bird really gets a sound in his mind from hearing it and sets out forthwith to imitate it, as mocking-birds are said at times to do, it is a mystery and deserves closest study. If a bird, out of a lot of random noises that it makes, chooses those for repetition which are like sounds that he has heard, it is again a mystery *why*, though not as in the previous case a mystery *how*, he does it. The important fact for our purpose is that, though the imitation of sounds is so habitual, there does not appear to be any marked general imitative tendency in these birds. There is no proof that parrots do muscular acts from having seen other parrots do them. But this should be studied. At any rate, until we know what sort of sounds birds imitate, what circumstances or emotional attitudes these are connected with, how they learn them and, above all, whether there is in birds which repeat sounds any tendency to imitate in other lines, we cannot, it seems to me, connect these phenomena with anything found in the mammals or use them to advantage in a discussion of animal imitation as the forerunner of human. In what follows they will be left out of account, will be regarded as a specialization removed from the general course of mental development, just as the feathers or right aortic arch of birds are particular specializations of no consequence for the physical development of mammals. For us, henceforth, imitation will mean imitation minus the phenomena of imitative birds.

There are also certain pseudo-imitative or semi-imitative phenomena which ought to be considered by themselves. For example, the rapid loss of the fear of railroad trains or telegraph wires among birds, the rapid acquisition of arboreal habits among Australian rodents, the use of proper feeding grounds, etc., may be held to be due to imitation. The young animal stays with or follows its mother from a specific instinct to keep near that particular object, to wit, its mother. It may thus learn to stay near trains, or scrabble up trees, or feed at certain places and on certain plants. Actions due to following pure and simple may thus simulate imitation. Other groups of acts which now seem truly imitative may be indirect fruits of some one instinct. This must be kept in mind when one estimates the supposed imitation of parents by young. Further, it is certain that in the case of the chick, where early animal life has been carefully observed, instinct and individual experience between them rob imitation of practically all its supposed influence. Chicks get along without a mother very well. Yet no mother takes more care of her children than the hen. Care in other cases, then, need not mean instruction through imitation.

These considerations may prevent an unreserved acceptance of the common view that young animals get a great number of their useful habits from imitation, but I do not expect or desire them to lead to its summary rejection. I should not now myself reject it, though I think it quite possible that more investigation and experiment may finally reduce all the phenomena of so-called imitation of parents by young to the level of indirect results of instinctive acts.

Another special department of imitation may be at least vaguely marked off: namely, apparent imitation of certain limited sorts of acts which are somewhat frequent in the animal's life. An example will do better than further definition.

Some sheep were being driven on board ship one at a time. In the course of their progress they had to jump over a hurdle. On this being removed before all had passed it, the next sheep was seen to jump as if to get over a hurdle, and so on for five or six, apparently sure evidence that they imitated the action, each of the one in front. Now, it is again possible that among gregarious animals there may be elaborate connections in the nervous system which allow the sight of certain particular acts in another animal to arouse the innervation leading to those acts, but that these connections are *limited.* The reactions on this view are specific responses to definite signals, comparable to any other instinctive or associational reaction. The sheep jumps when he sees the other sheep jump, not because of a general ability to do what he sees done, but because he is furnished with the instinct to jump at such a sight, or because his experience of following the flock over boulders and brooks and walls has got him into the habit of jumping at the spot where he sees one ahead of him jump; and so he jumps even though no obstacle be in his way. If due to instinct the only peculiarity of such a reaction would be that the sense-impression calling forth the act would be the same act as done by another. If due to experience there would be an exact correspondence to the frequent acts called forth *originally* by several elements in a sense-impression, one of which is essential, and done *afterwards* when only the *non-essentials* are present. These two possibilities have not been sufficiently realized, yet they may contain the truth. On the other hand, these limited acts may be the primitive, sporadic beginnings of the general imitative faculty which we find in man. To this general faculty we may now turn, having cleared away some of the more doubtful phenomena which have shared its name.

It should be kept in mind that an imitative act may be performed quite unthinkingly, as when a man in the mob shouts what the others shout or claps when the others clap; may be done from an inference that since A by doing X makes pleasure for himself, I by doing X may get pleasure for myself; may, lastly, be done from what may be called a transferred association. This process is the one of interest in connection with our general topic, and most of my experiments on imitation were directed to the investigation of it. Its nature is simple. One sees the following sequence: 'A turning a faucet, A getting a drink.' If one can free this association from its narrow confinement to A, so as to get from it the association, 'impulse to turn faucet, *me* getting a drink,' one will surely, if thirsty, turn the faucet, though he had never done so before. If one can from an act witnessed learn to do the act, he in some way makes use of the sequence seen, transfers the process to himself; in the common human sense of the word, he *imitates*. This kind of imitation is surely common in human life. It may be apparent in ontogeny before any power of inference is shown. After that power does appear, it still retains a wide scope, and teaches us a majority, perhaps, of the ordinary accomplishments of our practical life.

Now, as the writers of books about animal intelligence have not differentiated this meaning from the other possible ones, it is impossible to say surely that they have uniformly credited it to animals, and it is profitless to catalogue here their vague statements. Many opposers of the 'reason' theory have presupposed such a process and used it to replace reason as the cause of some intelligent performances. The upholders of the reason theory have customarily recognized such a process and claimed to have discounted it in their explanations of the various anecdotes. So we found Mr. Romanes, in the passage quoted, discussing the possibility that such an imitative process, without reason, could account for the facts. In his chapter on Imitation in 'Habit and Instinct,' Principal C. Lloyd Morgan, the sanest writer on comparative psychology, seems to accept imitation of this sort as a fact, though he could, if attacked, explain most of his illustrations by the simple forms. The fact is, as was said before, that no one has analyzed or systematized the phenomena, and so one cannot find clear, decisive statements to quote.

At any rate, whether previous authorities have agreed that such a process is present or not, it is worth while to tackle the question; and the formation of associations by imitation, if it occurs, is an important division of the formation of associations in general. The experiments and their results may now be described.

Imitation in Chicks.

No. 64 learned to get out of a certain pen (16 x 10 inches) by crawling under the wire screening at a certain spot. There was also a chance to get out by walking up an inclined plane and then jumping down. No. 66 was put in with 64. After 9 minutes, 20 seconds, 66 went out by the inclined plane, although 64 had in the mean time crawled out under the screen 9 times. (As soon as he got out and ate a little he was put back.) It was impossible to judge how many of these times 66 really saw 64 do this. He was looking in that direction 5 of the times. So also, in three more trials, 66 used the inclined plane, though 64 crawled under each time. 67 was then tried. In 4 minutes, 10 seconds, he crawled under, 64 having done so twice. Being then put in *alone*, he, without the chance to imitate, still crawled under. So probably he went under *when with 64* not by imitation but by accident, just as 64 had learned the thing himself.

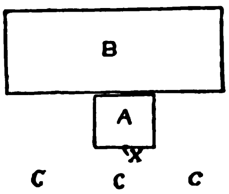


Fig. 17.

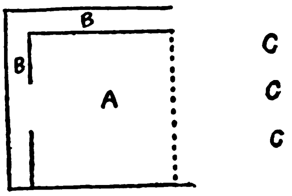


Fig. 18.

The accompanying figure (17) shows the apparatus used in the next experiment. A represents the top of a box (5 x 4 inches), 13 inches above the level of the floor, C. On the floor C were the chicks and food. B is the top of a box 10 inches high. Around the edges of A except the one next B a wire screen was placed, and 65 was repeatedly put upon A until he learned to go quickly back to C *via* B. Then the screen was bent outward at X so that a chick could barely squeeze through and down (A to C). Eleven chicks were then one at a time placed on A with 65. In every case but one they went A–C. In the case of the chick (75) who went A–B–C, there could have been no imitation, for he went down *before* 65 did. One other went through the hole before 65 went to B. The remaining nine all had a chance to imitate 65 and to save the uncomfortable struggle to get through the hole, 65 going A–B–C 8 times before 68 went A–C, 2 times when with 66 and 76, once in the case of each of the others.

In still another experiment the apparatus was (as shown in figure 18) a pen 14 inches square, 10 inches high, with a wire screen in front and a hole 3½ inches square in the back. This hole opened into a passage-way (B) leading around to C, where were the other chicks and food. Chicks who had failed, when put in alone, to find the way out, were put in with other chicks who had learned the way, to see if by seeing them go out they would learn the way. Chick 70 was given 4 trials alone, being left in the box 76 minutes all told. He was then given 9 trials (165 minutes) with another chick who went out *via* B 36 times. 70 failed to follow him on any occasion. The trials were all given in the course of two days. Chick 73 failed in 1 trial (12 minutes) to get out of himself, and was then given 4 trials (94 minutes) with another chick who went out *via* B 33 times. In this experiment, as in all others reported, sure evidence that the animals wanted to get out, was afforded by their persistent peckings and jumpings at the screen or bars that stood between them and C. Chick 72, after 8 unsuccessful trials alone (41 minutes), was given 8 trials with a chance to imitate. After the other chick had gone out 44 times, 72 *did go out.* He did not follow the other but went 20 seconds later. It depends upon one's general opinion whether one shall attribute this one case out of three to accident or imitation.

I also took two chicks, one of whom learned to escape from A (in figure 17) by going to B and jumping down the side to the *right* of A, the other of whom learned to jump down the side to the *left*, and placed them together upon A. Each took his own course uninfluenced by the other in 10 trials.

Chicks were also tried in several pens where there was only one possible way of escape to see if they would learn it *more quickly* when another chick did the thing several times before their eyes. The method was to give some chicks their first trial with an imitation possibility and their second without, while others were given their first trial without and their second with.

If the ratio of the average time of the first trial to the average time of the second is smaller in the first class than it is in the second class, we may find evidence of this sort of influence by imitation. Though imitation may not be able to make an animal *do* what he would otherwise *not do*, it may make him do *quicker* a thing he would have done sooner or later any way. As a fact the ratio is ***much larger***. This is due to the fact that a chick, when in a pen with another chick, is not afflicted by the discomfort of loneliness, and so does not try so hard to get out. So the other chick, who is continually being put in with him to teach him the way out, really prolongs his stay in. This factor destroys the value of these quantitative experiments, and I do not insist upon them as evidence against imitation, though they certainly offer none for it. I do not give descriptions of the apparatus used in these experiments or a detailed enumeration of the results, because in this discussion we are not dealing primarily with imitation as a slight general factor in forming experience, but as a definite associational process in the mind. The utter absence of imitation in this limited sense is apparently demonstrated by the results of the following experiments.

V was a box 16 x 12 x 8½, with the front made of wire screening and at the left end a little door held by a bolt but in such a way that a sharp peck at the top of the door would force it open.

W was a box of similar size, with a door in the same place fixed so that it was opened by raising a bolt. To this bolt was tied a string which went up over the top of the edge of the box and back across the box, as in D. By jumping up and coming down with the head over this thread, the bolt would be pulled up. The thread was 8½ inches above the floor.

X was a box of similar size, with door, bolt and string likewise. But here the string continued round a pulley at the back down to a platform in the corner of the box. By stepping on the platform the door was opened.

Y was a box 12 x 8 x 8½, with a door in the middle of the front, which I myself opened when a chick pecked at a tack which hung against the front of the box 1½ inches above the top of the door.

These different acts, pecking at a door, jumping up and with the neck pulling down a string, stepping on a platform, and pecking at a tack, were the ones which various chicks were given a chance to imitate. The chicks used were from 16 to 30 days old. The method of experiment was to put a chick in, leave him 60 to 80 seconds, then put in another who knew the act, and on his performing it, to let both escape. No cases were counted unless the imitator apparently saw the other do the thing. After about every ten such chances to learn the act, the imitator was left in alone for ten minutes. The following table gives the results. The imitators of course had previously failed to form the association of themselves. F denotes failure to perform the act:

Chick.	Act.	No. Times Saw.	Times in Which Failed.	Final Time.
84	V	38	45.00 F	15.00 F
85	V	30	30.00 F	10.00 F
86	V	44	55.00 F	15.00 F
87	V	26	35.00 F	15.00 F
80	W	54	60.00 F	15.00 F
81	W	40	45.00 F	15.00 F
87	W	27	30.00 F	10.00 F
81	X	18	20.00 F	10.00 F
82	X	21	20.00 F	8.40 *Did*
83	X	33	35.00 F	15.00 F
84	X	46	55.00 F	15.00 F
84	Y	45	55.00 F	15.00 F
83	Y	29	35.00 F	15.00 F

Thus out of all these cases only one did the act in spite of the ample chance for imitation. I have no hesitation in declaring 82's act in stepping on the platform the result of mere accident, and am sure that any one who had watched the experiments would agree.

IMITATION IN CATS.

By reference to the previous descriptions of apparatus, it will be seen that box D was arranged with two compartments, separated by a wire screen. The larger of these had a front of wooden bars with a door which fell open when a string stretched across the top was bitten or clawed down. The smaller was closed by boards on three sides and by the wire screen on the fourth. Through the screen a cat within could see the one to be imitated pull the string, go out through the door thus opened and eat the fish outside. When put in this compartment, the top being covered by a large box, a cat soon gave up efforts to claw through the screen, quieted down and watched more or less the proceedings going on in the other compartment. Thus this apparatus could be used to test the power of imitation. A cat who had no experience with the means of escape from the large compartment was put in the closed one; another cat, who would do it readily, was allowed to go through the performance of pulling the string, going out, and eating the fish. Record was made of the number of times he did so and of the number of times the imitator had his eyes clearly fixed on him. These were called 'times seen.' Cases where the imitator was looking in the general direction of the 'imitatee' and might very well have seen him and probably did, were marked 'doubtful.' In the remaining cases the cat did not see what was done by his instructor. After the imitatee had done the thing a number of times, the other was put in the big compartment alone, and the time it took him before pulling the string was noted and his general behavior closely observed. If he failed in 5 or 10 or 15 minutes to do so, he was released and not fed. This entire experiment was repeated a number of times. From the times taken by the imitator to escape and from observation of the way that he did it, we can decide whether imitation played any part. The history of several cases are given in the following tables. In the first column are given the lengths of time that the imitator was shut up in the box watching the imitatee. In the second column is the number of times that the latter did the trick. In the third and fourth are the times that the imitator surely and possibly saw it done, while in the last is given the time that, when tried alone, the imitator took to pull the string, or if he failed, the time he was in the box trying to get out. Times are in minutes and seconds, failures denoted by F:

		No. 7 Imitating No. 2.			
	Time Watching.	No. of times 2 did.	No. of times 7 saw.	No. of times Doubtful.	Time of 7 when alone.
	10.00	11	3	5	
After 48 Hours	11.00	10	4	2	
	12.00	20	4	13	10.00 F
After 24 Hours	8.00	20	6	11	1.00[1] 3.30 10.00 F
	13.00	25	8	12	20.00 F
After 24 Hours	9.00	20	4	11	10.00 F
After 24 Hours	12.00	35	5	21	30.00 F
After 2 Hours	10.00	25	3	8	25.00 F
After 24 Hours	15.00	35	6	21	20.00 F
After 24 Hours	6.00	20	0	7	10.00 F
Total times surely and possibly seen, -			43	111	

		No. 5 Imitating No. 2.			
	Time Watching.	No. of times 2 did.	No. of times 5 saw.	No. of times Doubtful.	Time of 5 when alone.
	12.00	15	3	8	5.00 F
After 2 Hours	10.00	8	4	4	
After 24 Hours	5.00	5	0	3	
After 1 Hour	14.00	10	5	3	10.00 F
After 1 Hour	13.00	22	7	11	10.00 F
After 24 Hours	7.00	15	3	8	5.00 F
After 48 Hours	18.00	20	2	9	20.00 F
After 24 Hours	14.00	20	2	10	30.00 F
After 24 Hours	10.00	20	7	12	20.00 F
Total times surely and possibly seen, -			33	68	

		No. 6 Imitating No. 2.			
	Time Watching.	No. of times 2 did.	No. of times 6 saw.	No. of times Doubtful.	Time of 6 when alone.
	12.00	30	0	19	1.10[2]
After 48 Hours	11.00	30	0	11	9.30
After 72 Hours	10.00	30	0	15	3.00
After 72 Hours	6.00	20	3	7	1.50
After 24 Hours	9.00	30	1	13	10.00 F
After 24 Hours	10.00	30	6	9	10.00 F
After 24 Hours	10.00	30	1	8	9.40
Total times surely and possibly seen, -			11	82	

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[1] No. 7 hit the string in his general struggling, apparently utterly without design. He did not realize that the door was open till, two seconds after it had fallen, he happened to look that way.

[2] No. 6, in trying to crawl out at the top of the box, put its paw in above the string. It fell down and thus pulled the string. It did not claw *at* it, and it was 16 seconds before it noticed that the door was open. In all the other times that it escaped the movement was made in the course of promiscuous scrabbling, never in anything like the same way that No. 2 made it.

		No. 3 Imitating No. 2.			
	8.00	30	2	19	3.30[1] 3.30
After 48 Hours	10.00	30	2	14	.20 .20
After 72 Hours	10.00	30	2	8	.18 .08
Total time surely and possibly seen,		-	6	41	

Before entering upon a discussion of the facts shown by these tables, we must describe the behavior of the imitators, when, after seeing 2 pull the string, they were put in alone. In the opinion of the present observer there was not the slightest difference between their behavior and that of cats 4, 10, 11, 12 and 13, who were put into the same position without ever having seen 2 escape from it. 6, 7, 5 and 3 paid no more attention to the string than they did, but struggled in just the same way. No one, I am sure, who had seen them, would have claimed that their conduct was at all influenced by what they had seen. When they did hit the string the act looked just like the accidental success of the ordinary association experiment. But, besides these personal observations, we have in the impersonal time-records sufficient proofs of the absence of imitation. If the animals pulled the string from having seen 2 do so, they ought to pull it in each individual case at an approximately regular length of time after they were put in, and presumably pretty soon thereafter. That is, if an association between the sight of that string in that total situation and a certain impulse and consequent freedom and food had been formed in their minds by the observation of the acts of 2, they ought to pull it *on seeing it*, and if any disturbing factor required that a certain time should elapse before the imitative faculty got in working order, that time ought to be somewhere near constant. The times were, as a fact, long and irregular in the extreme. Furthermore, if the successful cases were even in part due to imitation, the times ought to decrease the more they saw 2 do the thing. Except with 3, they *increase* or give place to failures. Whereas 6 and 7, if they had been put in again immediately after their first successful trial and from then on repeatedly, would have unquestionably formed the association, they did not, when put in after a further chance to increase their knowledge by imitation, do the thing as soon as before. The case of 3 is not here comparable to the rest because he *was* given three trials in immediate succession. He was a more active cat and quicker to learn, as may be seen by comparing his time curves with those of 7, 6 and 5. That the mere speed with which he mastered this association is no sign that imitation was present may be seen by reference to the time curves of 4 and 13 (on page 18).

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[1] No. 3 did not go out until 12 seconds had elapsed after it had pulled the string.

Some cats were also experimented with in the following manner. They were put into a box [No. 7 into box A (o at front), No. 5 into B (o at back)] and left for from 45 to 75 seconds. Then a cat who knew the way to get out was put in, and, of course, pulled at the loop and opened the door. *Both cats then went out and both were fed.* After the cat had been given a number of such chances to learn by imitation, he was put in and left until he did the thing, or until 5 or 10 minutes elapsed. As in the preceding experiments, no change in their behavior which might signify imitation was observed. No. 7 acted exactly like 3, or 10, or 11, when put in the box, apparently forming the association by accident in just the same way. Good evidence that he did not imitate is the fact that, whereas 1 (whom he saw) pulled the loop with his teeth, 7 pulled it with his paw. 5 failed to form the association, though he saw 3 do it 8 times and probably saw him 18 times more. He did get out twice by clawing the ***string*** in the ***front*** of the box, not the ***loop*** in the ***back***, as 3 did. These successes took place early in the experiment. After that he failed when left alone to get out at all.

Another experiment was made by a still different method. My cats were kept in a large box about 4 ft. high, the front of which was covered with poultry-yard netting. Its top was a board which could be removed. To save opening the door and letting them all loose, I was in the habit of taking them out by the top when I wanted to experiment with them. Of course the one who happened to climb up (perhaps attracted by the smell of fish on my fingers) was most likely to be taken out and experimented with and fed. Thus they formed the habit of climbing up the front of the box whenever I approached. Of three cats which I obtained at the same time, one did not after 8 or 10 days acquire this habit. Even though I held out a piece of fish through the netting, he would not climb after it. It was reasonable to suppose that imitation might overcome this sluggishness, if there were any imitation. I therefore put two cats with him and had them climb up 80 times before his eyes and get fish. He never followed or tried to follow them.

4 and 3 had been subjected to the following experiment. I would make a certain sound and after 10 seconds would go up to the cage and hold the fish out to them through the netting at the top. They would then, of course, climb up and eat it. After a while, they began to climb up upon hearing the signal (4) or before the 10 seconds were up. I then took 12 and 10, who were accustomed to going up when they saw me approach, but who had no knowledge of the fact that the signal meant anything, and gave them each a chance to imitate 3. That is one of them would be left in the box with 3, the signal would be given, and after from 5 to 10 seconds 3 would climb up. At 10 seconds I would come up with food, and then of course 12 would climb up. This was repeated again and again. The question was whether imitation would lead them to form the association more quickly than they would have done alone. It did not. That when at last they did climb up before 10 seconds was past, that is before I approached with food, it was not due to imitation, is shown by the fact that on about half of such occasions they climbed up *before 3 did.* That is they reacted to the *signal* by *association*, not to his *movements* by *imitation.*

IMITATION IN DOGS.

Here the method was not to see if imitation could arouse more quickly an act which accident was fairly likely to bring forth sooner or later, but to see if, where accident failed, imitation would succeed.

3 was found to be unable of himself to escape from box BB1, and was then given a chance to learn from watching 1. The

back of box BB1 was torn off and wire netting substituted for it. Another box with open front was placed directly behind and against box BB1. No. 3, who was put in this second box, could thus see whatever took place in and in front of box BB1 (o at back, high). The record follows:

		Dog 3 Imitating Dog 1.		
	Times 1 did.	Times 3 saw.	Times probably saw.	Times in alone.
	30	7	14	3.00 F
After 1 Hour - -	35	9	14	3.00 F
After 1 Hour - -	10	3	3	5.00 F
After 24 Hours - -	20	6	8	
	30	8	13	6.00 F
After 48 Hours - -	25	8	11	8.00 F
	25	6	12	6.00 F
	25	9	7	10.00 F
After 24 Hours - -	30	10	11	40.00 F
Total times surely and possibly seen, -		66	93	

A similar failure to imitate was observed in the case of another simple act. No. 1, as may be seen on page 33, had learned to escape from a pen about 8 by 5 feet by jumping up and biting a cord which ran from one end of the pen to the other and at the front end was tied to the bolt which held the door. Dogs 2 and 3 had failed in their accidental jumping and pawing to hit this cord, and were then given a chance to learn by seeing 1 do so, escape, and of course be fed. 1 always jumped in the same way, biting the cord at the same place, namely where a loose end from a knot in it hung down 4 or 5 inches. 2 and 3 would either be tied up in the pen or left in a pen at one side. They had a perfect chance to see 1 perform his successful act. After every twenty or thirty performances by 1, 2 and 3 would be put in alone. It should be remembered that here, as also in the previous experiment and all others, the imitators certainly *wanted* to get out when thus left in alone. They struggled and jumped and pawed and bit, but they never jumped *at the cord.* Their records follow.

		Dog 2 Imitating Dog 1.		
	Times 1 did.	Times 2 saw.	Times Doubtful.	Time 2 was in alone.
	30	9	11	10.00 F
After 1 Hour, - -	30	10	9	10.00 F
After 48 Hours, - -	25	8	8	
After 1 Hour, - -	10	3	4	9.00 F[1]
After 24 Hours, - -	30	8	12	15.00 F
After 1 Hour, - -	30	9	12	15.00 F
After 48 Hours, - -	20	7	6	10.00 F
	20	8	7	
After 48 Hours, - -	30	6	8	15.00 F
After 24 Hours, - -	15	2	4	10.00 F
Total times surely and possibly seen, -		70	81	

		Dog 3 Imitating Dog 1.		
	Time 1 did.	Times 3 saw.	Times Doubtful.	Time 3 was in alone.
	30	10	10	10.00 F
After 1 Hour, - -	30	9	10	10.00 F
After 1 Hour, - -	15	6	4	
After 24 Hours, - -	30	9	11	15.00 F
After 24 Hours, - -	30	10	12	15.00 F
After 1 Hour, - -	30	8	9	10.00 F
After 48 Hours, - -	20	6	7	40.00 F
After 1 Hour, - -	20	6	5	
After 48 Hours, - -	30	8	9	15.00 F
After 24 Hours, - -	15	3	4	20.00 F
Total times surely and possibly seen, -		75	81	

Another corroborative, though not very valuable, experiment was the following: Dog 3 had been taught for the purpose of another experiment to jump up on a box and beg when I held a piece of meat above the box. I then caused him to do this 110 times (within two days) in the presence of 1. Although 1 saw him at least 20 % of the times (3 was always fed each time he jumped on the box), he never tried to imitate him.

It seems sure from these experiments that the animals were unable to form an association leading to an act from having seen the other animal, or animals, perform the act in a certain situation. Thus we have further restricted the association process. Not only do animals not have associations accompanied, more or less permeated and altered, by inference and judgment; they do not have associations of the sort which may be acquired from other animals by imitation. What this implies concerning the actual mental content accompanying their acts will be seen later on. It also seems sure that we should give up imitation as an *a priori* explanation of any novel intelligent performance. To say that a dog who opens a gate, for instance, need not have reasoned it out *if he had seen another dog do the same thing*, is to offer instead of one false explanation another equally false. Imitation in any form is too doubtful a factor to be presupposed without evidence. And if a general imitative faculty is not sufficiently developed to succeed with such simple acts as those of the experiments quoted, it must be confessed that the faculty is in these higher mammals still rudimentary and capable of influencing to only the most simple and habitual acts, or else that for some reason its sphere of influence is limited to a certain class of acts, possessed of some *qualitative difference*, other than mere simplicity, which renders them imitable. The latter view seems a hard one to reconcile with a sound psychology of imitation or association at present, without resorting to instinct. Unless a certain class of acts are by the innate mental make-up especially tender to the influence of imitation, the theory fails to find good psychological ground to stand on. The former view may very well be true. But in any case the burden of proof would now seem to rest upon the adherents to imitation; the promising attitude would seem to be one which went without imitation as long as it could, and that is, of course, until it surely found it present.

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[1] The back of the pen adjoined the elevator shaft, being separated from it by a partition 33 inches high. No. 2 heard the elevator coming up and put his paws up on the top of this partition so as to look over. In so doing he knocked the fastening of the cord at that end and opened the door. He did not turn to come out, and I shut the door again.

Returning to imitation considered in its human aspect, to imitation as a transferred association in particular, we find that here our analytical study of the animal mind promises important contributions to general comparative psychology. If it is true, and there has been no disagreement about it, that the primates do imitate acts of such novelty and complexity that only this out-and-out kind of imitation can explain the fact, we have located one great advance in mental development. Till the primates we get practically nothing but instincts and individual acquirement through impulsive trial and error. Among the primates we get also acquisition by imitation, one form of the increase of mental equipment by tradition. The child may learn from the parent quickly without the tiresome process of seeing for himself. The less active and less curious may share the progress of their superiors. The brain whose impulses hitherto could only be dislodged by specific sense-impressions may now have any impulse set agoing by the sight of the movement to which it corresponds.

All this on the common supposition that the primates *do* imitate, that a monkey in the place of these cats and dogs *would* have pulled the string. My apology for leaving the matter in this way without experiments of my own is that the monkey which I procured for just this purpose failed in two months to become tame enough to be thus experimented on. Accurate information about the nature and extent of imitation among the primates should be the first aim of further work in comparative psychology, and will be sought by the present writer as soon as he can get subjects fit for experiments.

In a questionnaire which was sent to fifteen animal-trainers, the following questions were asked:

1. "If one dog was in the habit of 'begging' to get food and another dog saw him do it ten or twenty times, would the second dog then beg himself?"

2. "In general is it easier for you to teach a cat or dog a trick if he has seen another do it?" •

3. "In general do cats imitate each other? Do dogs? Do monkeys?"

4. "Give reasons for your opinion, and please write all the reasons you have."

Five gentlemen (Messrs. R. C. Carlisle, C. L. Edwards, V. P. Wormwood, H. S. Maguire and W. E. Burke) courteously responded to my questionnaire. All are trainers of acknowledged reputation. To these questions on imitation four replied.

To the first question we find the following answers: (*a*) "Most dogs would." (*b*) "Yes; he will very likely do it. He will try and imitate the other dog *generally*." (*c*) "If a young dog with the mother, it would be very apt to . . . With older dogs, it would depend very much upon circumstances." (*d*) "He would not."

To 2 the answers were: (*a*) "Very much easier." (*b*) "It is always easier if they see another one do it often." (*c*) "This would also depend on certain conditions. In teaching to jump out of a box and in again, seeing another might help, but in teaching something very difficult, I do not think it would be the case." (*d*) "It is not."

To 3 the answers were: (*a*) "Yes. Some. More than either dogs or cats." (*b*) "Yes. Yes. Yes." (*c*) "In certain things, yes; mostly in those things which are in compliance to the laws of their own nature." (*d*) "No. No. Yes, they are born imitators."

The only definite answer to question 4 was: "Take a dog or cat and close them up in a room and go in and out several times, and you will find that they will go to the door and stand up on their hind legs with front paws on the door knob and try to open the door to get out. I could also give you a hundred more such reasons." This was given by (*b*).

The replies to a test question, however, go to show that these opinions regarding imitation may be mistaken. Question 8 was: "If you wanted to teach a cat to get out of a cage by opening an ordinary thumb-latch and then pushing the door, would you take the cat's paw and push down the thumb-piece with it and then push the door open with the paw, or would you just leave the cat inside until it learned the trick itself?" The second is certainly the better way, as will be seen in a later part of this paper, and pushing the latch with the cat's paw has absolutely no beneficial influence on the formation of the association, yet (*a*) and (*b*) both chose the first way, and (*c*) answered ambiguously. Further, the only reason given is, of course, no reason at all. It proves too much, for if there were such imitation as that, my cats and dogs would surely have done the far simpler things required of them. I cannot find that trainers make any practical use of imitation in teaching animals tricks, and on the whole I think these replies leave the matter just where it was before. They are mere opinions—not records of observed facts. It seems arrogant and may seem to some unjustifiable thus to discard testimony, to stick to a theory based on one's own experiments in the face of these opinions. If I had wished to gain applause and avoid adverse criticism, I would have abstained from upholding the radical view of the preceding pages. At times it seems incredible to me that the results of my experiments should embody the truth of the matter, that there should be no imitation. The theory based on them seems, even to me, too radical, too novel. It seems highly improbable that I should be right and all the others wrong. But I cannot avoid the responsibility of giving what seems to my judgment the most probable explanation of the results of the experiments; and that is the radical explanation already given.

THE MENTAL FACT IN ASSOCIATION.

It is now time to put the question as to just what is in an animal's mind when, having profited by numerous experiences, he has formed the association and does the proper act when put in a certain box. The commonly accepted view of the mental fact then present is that the sight of the inside of the box reminds the animal of his *previous pleasant experience after escape* and *of the movements* which he made which were immediately followed by and so associated with that escape. It has been taken for granted that *if the animal remembered the pleasant experience and remembered the movement, he would make the movement.* It has been assumed that the association was *an association of ideas;* that when one of the ideas was of a movement the animal was capable of making the movement. So, for example, Morgan says, in the 'Introduction to Comparative Psychology': "If a chick takes a lady-bird in its beak forty times and each time finds it nasty, this is of no practical value to the bird unless the sight of the bird suggests *the nasty taste*," p. 90.

Again, on page 92, Morgan says, "*A race after the ball* had been suggested through the channel of olfactory sensations." Also on page 86 "* * * the visual impression suggested the idea or representation of unpleasant gustatory experience." The attitude is brought out more completely in a longer passage on page 118: "On one of our first ascents one of them put up a young coney, and they both gave chase. Subsequently they always hurried on to this spot, and, though they never saw another coney there, reiterated disappointment did not efface *the memory of that first chase*, or so it seemed." That is, according to Morgan, the dogs thought of the chase and its pleasure, on nearing the spot where it had occurred, and so hurried on. On page 148 of 'Habit and Instinct,' we read, "Ducklings so thoroughly associated water with the sight of their tin that they tried to drink from it and wash in it when it was empty, nor did they desist for some minutes," and this with other similar phenomena is attributed to the 'association by contiguity' of human psychology.

From these quotations it seems fairly sure that if we should ask Mr. Morgan, who is our best comparative psychologist, what took place in the mind of one of these cats of our experiments during the performance of one of the 'tricks,' he would reply: "The cat performs the act because of the association of ideas. He is reminded by the sight of the box and loop of his experience of pulling that loop and of eating fish outside. So he goes and pulls it again." This view has stood unchallenged, but its implication is false. It implies that an animal, whenever it thinks of an act, can supply an *impulse to do* the act. It takes for granted that the performance of a cat who gets out of a box is mentally like that of a man who thinks of going down street or of writing a letter and then does it. The mental process is not alike in the two cases, for animals can *not* provide the impulse to *do* whatever act they think *of*. *No cat can form an association leading to an act unless there is included in the association an impulse of its own which leads to the act.* There is no general storehouse from which the impulse may be supplied after the association is formed.

Before describing the experiments which justify these statements, it will be worth while to recall the somewhat obvious facts about the composition of one of these associations. There might be in an association, such as is formed after experience with one of our boxes, the following elements:

1. Sense-impression of the interior of the box, etc.
2. (*a*) Discomfort and (*b*) desire to get out.
3. Representation of oneself pulling the loop.
4. Fiat comparable to the human "I'll do it."
5. The impulse which actually does it.
6. Sense-impression of oneself pulling the loop, seeing one's paw in a certain place, feeling one's body in a certain way, etc.
7. Sense impression of going outside.
8. Sense-impression of eating, and the included pleasure.

Also between 1 and 4 we may have 9, representations of one's experience in going out, 10, of the taste of the food, etc.

6, 7 and 8 come after the act and do not influence it, of course, except in so far as they are the basis of the future 3's, 9's and 10's. About 2 we are not at present disputing. Our question is as to whether 3 or 5 is the essential thing. In human associations 3 certainly often is, and the animals have been credited with the same kind. Whatever he *thinks*, Professor Morgan surely *talks* as if 1 aroused 9 and 10 and 3 and leaves 5 to be supplied at will. We have affirmed that 5 is the essential thing, that no association without a specific 5 belonging to it and acquired by it can lead to an act. Let us look at the reasons.

A cat has been made to go into a box through the door, which is then closed. She pulls a loop and comes out and gets fish. She is made to go in by the door again, and again lets herself out. After this has happened enough times, the cat will of her own accord go into the box after eating the fish. It will be hard to keep her out. The old explanation of this would be that the cat associated the memory of being in the box with the subsequent pleasure, and therefore performed the equivalent of saying to herself, "Go to! I will go in." The thought of *being in*, they say, makes her *go in*. *The thought of being in will not make her go in.* For if, instead of pushing the cat toward the doorway or holding it there, and thus allowing it to itself give the impulse, to innervate the muscles, to walk in, you shut the door first and drop the cat in through a hole in the top of the box, she will, after escaping as many times as in the previous case, *not* go into the box of her own accord. She has had exactly the same opportunity of connecting the idea of being in the box with the subsequent pleasure. Either a cat cannot connect ideas, representations, at all, or she has not the power of progressing from the thought of being in to the act of going in. The only difference between the first cat and the second cat is that the first cat, in the course of the experience, has the impulse to crawl through that door, while the second has not the impulse to crawl through the door or to drop through that hole. So, though you put the second cat on the box beside the hole, she doesn't try to get into the box through it. The impulse is the *sine qua non* of the association. The second cat has everything else, but cannot supply that. These phenomena were observed in six cats, three of which were tried by the first method, three by the second. Of the first three, one went in himself on the 26th time and frequently thereafter, one on the 18th and the other on the 37th; the two last as well as the first did that frequently in later trials. The other three all failed to go in themselves after 50, 60 and 75 trials, respectively.

The case of No. 7 was especially instructive, though not among these six. No. 7 had had some trials in which it was put in through the door, but ordinarily in this particular experiment was dropped in. After about 80 trials it would frequently exhibit the following phenomena: It would, after eating the fish, go up to the doorway and, rushing from it, search for fish. The kitten was very small and would go up into the doorway, whirl round and dash out, all in one quick movement. The best description of its behavior is the paradoxical one that it went out without going in. The association evidently concerned what it had *done*, what it had an impulse for, namely, *coming out through that door* to get fish, not what it remembered, had a representation of.

Still more noteworthy evidence is found in the behavior of cats and dogs who were put in these boxes, left one or two minutes, and then put through the proper movement. For example, a cat would be put in B (o at back) and left two minutes. I would then put my hand in through the top of the box, take the cat's paw and with it pull down the loop. The cat would then go out and eat the fish. This would be done over and over again, and after every ten or fifteen such trials the cat would be left in alone. If in ten or twenty minutes he did not escape, he would be taken out through the top and not fed. In one series of experiments animals were taken and thus treated in boxes from which their own impulsive activity had failed to liberate them. The results, given in the table below, show that no animal who fails to perform an act in the course of his own impulsive activity will learn it by being put through it.

In these experiments some of the cats and all of the dogs but No. 1 showed no agitation or displeasure at my handling from the very start. Nor was there any in Dog 1 or the other cats after a few trials. It may also be remarked that in the trials alone which took place during and at the end of the experiment the animals without exception showed that they did not fail to perform the act from lack of a desire to get out. They all tried hard enough to get out and would surely have used the association if they had formed it.

Individual.	Apparatus.	Time in which impulsive activity failed to lead to the act.	Number of times the animal was put through the movement.	Time in which this experience failed to lead to the act.	Time of final trial.
Cat 1	F (String outside unfastened)	55.00	77	120.00	20.00
Cat 5	G (Thumb-latch) - -	57.00	59	55.00	10.00
Cat 7	G (Thumb-latch) - -	50.00	30	35.00	10.00
Cat 2	G (Thumb-latch) - -	54.00	141	110.00	20.00
Dog 2	BB1 (o at back, high) -	48.00	30	80.00	60.00
Dog 3	BB1 (o at back, high) -	20.00	85	55.00	10.00
Dog 2	M (Lever outside) - -	15.00	95	140.00	30.00
Dog 1	FF[1] - - - - -	30.00	110	135.00	60.00
Chick 89	X (See page 53).	20.00	30	60.00	30.00
Cat 13	KKK[2], [3] - - - -	40.00	65	60.00	10.00

Now, the only difference between the experiences of the animals in these experiments and their experiences in those where they let themselves out, is that here they only saw and felt themselves making the movement, whereas in the other case they also felt the impulse, gave the innervation. That, then, is the essential. It may be objected that the animals failed because they did not *attend* to the process of being put through the movement, that, had they attended to it, they would later themselves have made the movement. It is, however, improbable that out of fifty times an animal should not have attended to what was going on at least two or three times. But if seeing himself do it was on a par with feeling an impulse to and so doing it, even two or three times would suffice to start the habit. And it is even more improbable that an experience should be followed by keen pleasure fifty times and not be attended to with might and main, unless animals attend *only* to their own impulses and the excitements thereof. But if the latter be true, it simply affirms our view from a more fundamental standpoint.

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[1] FF was a box 40 × 21 × 24 inches, the door of which could be opened by putting the paw out between the bars to its right and pulling a loop which hung 16 inches above the floor, 4 inches out from the box and six inches to the right of the door.

[2] KKK was box K with both bolts removed. All that had to be done was to poke the paw out at one side of the door and press down a little bar of wood.

[3] The cats and chick were left in for two minutes at each trial, the dogs for from one to one and a half minutes.

In another set of experiments animals were put in boxes with whose mechanism they had had no experience, and from which they might or might not be able to escape by their own impulsive acts. The object was to see whether the time taken to form the association could be altered by my instruction. The results turned out to give a better proof of the inability to form an association by being put through the act than any failure to change the time-curve. For it happened in all but one of the cases that the movement which the animal made to open the door was different from the movement which I had put him through. Thus, several cats were put through (in Box C [Button]) the following movement: I took the right paw and, putting it against the lower right-hand side of the button, pushed it round to a horizontal position. The cats' ways were as follows: No. 1 turned it by clawing vigorously at its top; No. 6, by pushing it round with his nose; No. 7, in the course of an indiscriminate scrabble at first, in later trials either by pushing with his nose or clawing at the top, settling down finally to the last method. Nos. 2 and 5 did it as No. 1 did. Cat 2 was tried in B (o at back). I took his paw and pressed the loop with it, but he formed the habit of clawing and biting the string at the top of the box near the front. No. 1 was tried in A. I pressed the loop with his paw, but he formed the habit of biting at it.

In every case I kept on putting the animal through the act every time, if at the end of two minutes (one in several cases) it had not done it, even after it had shown, by using a different way, that my instruction had no influence. I never succeeded in getting the animal to change its way for mine. Moreover, if any one should fancy that the animal really profited by my instruction so as to learn what result to attain, namely, the turning of a certain button, but chose a way of his own to turn it, he would be deluding himself. The time taken to learn the act with instruction was no shorter than without.

If, then, an animal happens to learn an act by being put through it, it is just happening, nothing more. Of course, you may *direct* the animal's efforts so that he will perform the act himself the sooner. For instance, you may hold him so that his accidental pawing will be sure to hit the vital point of the contrivance. But the animal can not form an association leading to an act unless the particular impulse to that act is present as an element of the association; he cannot supply it from a general stock. The groundwork of animal associations is not the association of *ideas*, but the association of idea or sense-impression with *impulse.*

In the questionnaire mentioned elsewhere, some questions were asked with a view to obtaining corroboration or refutation of this theory that an impulse or innervation is a necessary element in every association formed if that association leads to an act. The questions and answers were:

Question 1: "If you wanted to teach a horse to tap seven times with his hoof when you asked him, "How many days are there in a week?," would you teach him by taking his leg and making him go through the motions?"

A answered, "Yes! at first."

B answered, "No! I would not."

C answered, "At first, yes!"

D answered, "No!"

Question 2: "Do you think you *could* teach him that way, even if naturally you would take some other way?"

A answered, "In time, yes!"

B answered, "I think it would be a very hard way."

C answered, "Certainly I do."

D answered, "I do not think I could."

E answered, "Yes."

Question 3: "How would you teach him?"

A answered, "I should tap his foot with a whip, so that he would raise it, and reward him each time."

B answered, "I should teach him by the motion of the whip."

C answered, "First teach him by pricking his leg the number of times you wanted his foot lifted."

D answered, "You put figure 2 on blackboard and touch him on leg twice with cane, and so on."

E answered ambiguously.

It is noteworthy that even those who think they *could* teach an animal by putting him through the trick do not use that method, except at first. And what they really do then is probably to stimulate the animal to the reflex act of raising his hoof. The hand simply replaces the cane or whip as the means of stimulus. The answers are especially instructive, because the numerous counting tricks done by trained horses seem, at first, to be incomprehensible, unless the trainer can teach the horse by putting it through the movement the proper number of times. The counting tricks performed by Mascot, Professor Maguire's horse, were quoted to me by a friend as incomprehensible on my theory. The answers given above show how simple the thing really is. All the counting-tricks of all the intelligent horses depend on the fact that a horse raises his hoof when a certain stimulus is given. One simple reaction gives the basis for a multitude of tricks. In the same way other tricks, which at first sight seem to require that the animal should learn by being put through the movement, may depend on some simple reflex or natural impulse.

Another question was, "How would you teach a cat to get out of a box, the door of which was closed with a thumb-latch?"

A answered, "I should use a puff-ball as a plaything for the cat to claw at." This means, I suppose, that he would get the cat to claw at the puff-ball and thus direct its clawings to the vicinity of the thumb-piece.

B answered, "I would put the cat in and get it good and hungry and then open the door by lifting the latch with my finger. Then put some food that the cat likes outside, and she will soon try to imitate you and so learn the trick."

C answered, "I would first adjust all things in connection with the surroundings of the cat so they would be applicable to the laws of its nature, and then proceed to teach the trick."

I suppose this last means that he would fix the box so that some of the cat's instinctive acts would lead it to perform the trick. The answer given by *B* means apparently that he would simply leave the thing to accident, for any such imitation as he supposes is out of the question. At all events, none of these would naturally start to teach the trick by putting the animal through the motions, which, were it a possible way, would probably be a traditional one among trainers. On the whole, I see in these data no reason for modifying our dogma that animals cannot learn acts without the impulse.

Presumably the reader has already seen budding out of this dogma a new possibility, a further simplification of our theories about animal consciousness. The possibility is that animals may have *no images or memories at all, no ideas to associate.* Perhaps the entire fact of association in animals is the presence of sense-impressions with which are associated, by resultant pleasure, certain impulses, and that therefore, and therefore only, a certain situation brings forth a certain act. Returning to our analysis of the association, this theory would say that there was no (9) or (10) or (3) or (4), that the sense-impression gave rise, when accompanied by the feeling of discomfort, to the impulse (5) directly, without the intervention of any representations of the taste of the food, or the experience of being outside, or the sight of oneself doing the act. This theory might be modified so as to allow that the representations could be there, but to deny that they were necessary, were inevitably present, that the impulse was connected to the sense-impression through them. It would then claim that the effective part of the association was a direct bond between the situation and the impulse, but would not cut off the possibility of there being an aura of memories along with the process. It then becomes a minor question of interpretation which will doubtless sooner or later demand an answer. I shall not try to answer it now. The more radical question, the question of the utter exclusion of representative trains of thought, of any genuine association of *ideas* from the mental life of animals, is worth serious consideration. I confess that, although certain authentic anecdotes and certain experiments to be described soon, lead me to reject this exclusion, there are many qualities in animals' behavior which seem to back it up. If one takes his stand by a rigid application of the law of parsimony, he will find justification for this view which no experiments of mine can overthrow.

Of one thing I am sure, and that is that it is worth while to state the question and how to solve it, for although the point of view involved is far removed from that of our leading psychologists to-day, it cannot long remain so. I am sorry that I cannot pretend to give a final decision.

The view seems preposterous because, if an animal has sense-impressions when his brain is excited by currents starting in the end-organs, it seems incredible that he should not be conscious in imagination and memory by having similar excitations caused from within. We are accustomed to think of memory as the companion of sensation. But, after all, it is a question of fact whether the connections in the cat-brain include connections between present sensation-neuroses and past sensation-neuroses. The only connections may be those between the former and impulse-neuroses, and there is no authoritative reason why we should suppose any others unless they are demonstrated by the cat's behavior. This is just the point at issue. Such evidence as the phenomena of animals' dreams does not at all prove the presence of memory or imagination. A dog may very well growl in his sleep without any idea of a hostile dog. The impulse to growl *may* be caused by chance excitement of its own neurosis without any sensation-neurosis being concerned. *Acts* of recognition may have no *feelings* of recognition going with or causing them. A sense-impression of me gets associated in my dog's mind with the impulses to jump on me, lick my hand, wag his tail, etc. If, after a year, the connection between the two has lasted, he will surely jump on me, lick my hand and wag his tail, though he has not and never had any representation of me.

The only logical way to go at this question and settle it is, I think, to find some association the formation of which requires the presence of images, of ideas. You have to give an animal a chance to associate sense-impression A with sense-impression B and then to associate B with some act C so that the presence of B in the mind will lead to the performance of C. Presumably the representation of B, if present, will lead to C just as the sense-impression B did. Now, if the chance to associate B with A has been improved, you ought, when the animal is confronted with the sense-impression A, to get a revival of B and so the act C. Such a result would, if all chance to associate C with A had been eliminated, demonstrate the presence of representations and their associations. I performed such an experiment in a form modified so as to make it practicable with my animals and resources. Unfortunately, this modification spoils the crucial nature of the experiment and robs it of much of its authority. The experiment was as follows:

A cat was in the big box where they were kept (see page 58) very hungry. As I had been for a long time the source of all food, the cats had grown to watch me very carefully. I sat, during the experiment, about eight feet from the box, and would at intervals of two minutes clap my hands four times and say, "I must feed those cats." Of course the cat would at first feel no impulse except perhaps to watch me more closely when this signal was given. After ten seconds had elapsed I would take a piece of fish, go up to the cage and hold it through the wire netting, three feet from the floor. The cat would then, of course, feel the impulse to climb up the front of the cage. In fact, experience had previously established the habit of climbing up whenever I moved toward the cage, so that in the experiment the cat did not ordinarily wait until I arrived there with the fish. In this experiment:

A=The sense-impression of my movements and voice when giving the signal.

B=The sense-impression of my movements in taking fish, rising, walking to box, etc.

C=The act of climbing up, with the impulse leading thereunto.

The question was whether after a while A would remind the cat of B, and cause him to do C before he got the *sense-impression* of B, that is, before the ten seconds were up. If A leads to C through a memory of B, animals surely *can* have association of ideas proper, and probably often *do*. Now, as a fact, after from thirty to sixty trials, the cat does perform C immediately on being confronted by A or some seconds later, at all events before B is presented. And it is my present opinion that their action is to be explained by the presence, through association, of the idea B. But it is not impossible that A was associated *directly* with the impulse to C, although that impulse was removed from it by ten seconds of time. Such an association is, it seems to me, highly improbable, unless the neurosis of A, and with it the psychosis, continues until the impulse to C appears. But if it does so continue during the ten seconds, and thus get directly linked to C, we have exactly a representation, an image, a memory, in the mind for eight of those ten seconds. It does not help the deniers of images to substitute an image of A for an image of B. Yet, unless they do this they have to suppose that A comes and goes, and that after ten seconds C comes, and, passing over the intervening blank, willfully choses out A and associates itself with it. There are some other considerations regarding the behavior of the cats from the time the signal was given till they climbed up, which may be omitted in the hope that it will soon be possible to perform a decisive experiment. If an observer can make sure of the animal's attention to a sequence A——B, where B does not arouse any impulse to an act, and then later get the animal to associate B with C, leaving A out this time, he may then, if A, when presented anew, arouses C, bid the deniers of representations to forever hold their peace.

Another reason for allowing animals representations and images is found in the longer time taken to form the association between the act of licking or scratching and the consequent escape. If the associations in general were simply between situation and impulse and act, one would suppose that the situation would be associated with the impulse to lick or scratch as readily as with the impulse to turn a button or claw a string. Such is not the case. By comparing the curves for Z on pages 22, 23 with the others one sees that for so simple an act it takes a long time to form the association. This is not a final reason, for lack of attention, a slight increase in the time taken to open the door after the act was done, or an absence of preparation in the nervous system for connections between these particular acts and definite sense-impressions, may very well have been the cause of the difficulty in forming the associations. Nor is it certain that *ideas* of clawing loops would be easier to form than ideas of scratching or licking oneself. The matter is still open to question. But, as said before, my opinion would be that animals *do* have representations and that such are the beginning of the rich life of ideas in man. For the most part, however, such are confined to specific and narrow practical lines. There was no evidence that my animals habitually *did* form associations of ideas from their experience throughout, or that such were constantly revived without the spur of immediate practical advantage.

Before leaving the topic an account may be given of experiments similar to the one described above as performed on cats 3 and 4, which were undertaken with Cat 13 and Dogs 1, 2 and 3.

Cat 13 was fed with pieces of fish at the top of the wire netting 45 times, to accustom it to climbing up when it saw me come with fish. I then went through the same process as with 3 and 4, but at intervals of 60 to 90 seconds instead of 120. After 90 such trials it occasionally climbed up a little way, but though 135 trials in all were given it never made the uniform and definite reaction which 3 and 4 did. It reacted, when it reacted at all, at from 5 to 9 seconds after the signal. Whether age, weight, lack of previous habitual climbing when I approached, or a slowness in forming the association made the difference, is uncertain.

Dog 1 was experimented on in the following manner: I would put him in a big pen, 20–10 feet, and sit outside facing it, he watching me as was his habit. I would pound with a stick and say, "Go over to the corner." After an interval (10 seconds for 35 trials, 5 seconds for 60 trials) I would go over to the corner (12 feet off) and drop a piece of meat there. He, of course, followed and secured it. On the 6th, 7th, 16th, 17th, 18th and 19th trials he did perform the act before the 10 seconds were up, then for several times went during the two-minute intervals without regarding the signal, and finally abandoned the habit altogether, although he showed by his behavior when the signal was given that he was not indifferent to it.

Dogs 1, 2 and 3 were also given 95, 135 and 95 trials, respectively, the acts done being (1) standing up against the wire netting inclosing the pen, (2) placing the paws on top of a keg, and (3) jumping up onto a box. The time intervals were 5 seconds in each case. No dog of these ever performed the act before I started to take the meat to feed them, but they did show, by getting up if they were lying down, when the signal was given, or by coming to me if they were in some other part of the pen, that something was suggested to them by it. Why these cases differ from the cases of Cats 3 and 4 (10 and 12 also presented phenomena like those reported in the cases of 3 and 4) is an interesting though not very important question. The dogs were not kept so hungry as were the cats, and experience had certainly not rendered the particular impulses involved so sensitive, so ready to discharge. Dogs 2 and 3 were older. There is no reason to invoke any qualitative difference in the mental make-up of the animals until more illuminating experiments are made.

ASSOCIATION BY SIMILARITY AND THE FORMATION OF CONCEPTS.

What there is to say on this subject from the standpoint of my experiments will be best introduced by an account of the experiments themselves.

Dog 1 had escaped from AA (0 at front) 26 times. He was then put in BB (0 at back). Now, whereas 2 and 3, who were put in without previous experience with AA, failed to paw the loop in BB, No. 1 succeeded. His times were 7.00, .35, 2.05, .40, .32, .10, 1.10, .38, .10, .05, and from then on he pawed the loop as soon as put in the box. After a day or so he was put in BB1 (0 at back high). Although the loop was in a new position, his times were only .20, .10, .10, etc. After nine days he was put in a box arranged with a little wooden platform 2½ inches square, hung where the loop was in BB1. Although the platform resembled the loop not the least save in position, his times were only .10, .07, .05, etc.

From the curves given in Figure 19, which tell the history of 10, 11 and 12 in B1 (0 at back) after each had previously been familiarized with A (0 at front), we see this same influence of practice in reacting to one mechanism upon the time taken to react to a mechanism at all similar. It naturally takes a cat a longer time to accidentally claw a loop in the back than in the front, yet a comparison of these curves with those on page 18, Figure 2, shows the opposite to have been the case with 10, 11 and 12. The same remarkable quickness was noted in Cats 1 and 3 when put into B (o at back) after learning A (o at front). Moreover, the loops were not alike. The loop in A was of smaller wire, covered with a bluish thread, while the loop in B was covered with a black rubber compound, the diameter of the loop being three times that of A's loop.

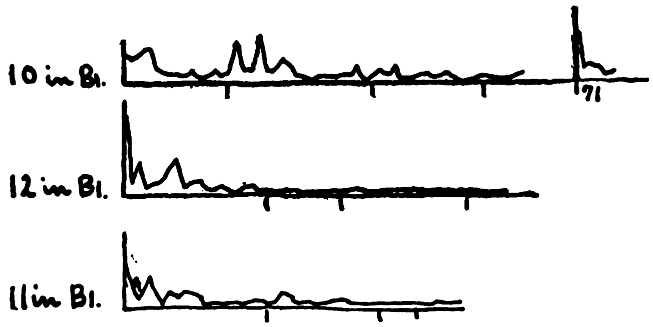

FIG. 19.

If any advocate of reason in animals has read so far, I doubt not that his heart has leaped with joy at these two preceding paragraphs. "How," he will say, "can you explain these facts without that prime factor in human reason, association by similarity? Surely they show the animal perceiving likenesses and acting from general ideas." *This is the very last thing that they show.* Let us see why they do not show this and what they do show. He who thinks that these animals had a general notion of a loop-like thing as the thing to be clawed, that they felt the loop in B, different as it was in size, color and position, to be still a loop, to have the essential quality of the other, must needs presuppose that the cat has a clear, accurate sensation and representation of both. Only if the cat discriminates can it later associate by noticing similarities. This is what such thinkers do presuppose. A bird, for instance, dives in the same manner into a river of yellow water, a pond or an ocean. It has a general notion, they say, of water. It knows that river water is one thing and pond-water another thing, but it knows that both are water, *ergo*, fit to dive into. The cat who reacts to a loop of small wire of a blue color knows just what that loop is, and when it sees a different loop knows its differences, but knows also its likeness, and reacts to the essential. Thus crediting the cat with our differentiation and perception of individuality, they credit it with our conceptions and perceptions of similarity. Unless the animal has the first there is no reason to suppose the last. Now, *the animal does not have either*. It does not in the first place react to that particular loop in A, with recognition of its qualities. It reacts to a vague, ill-defined sense-impression, undiscriminated and even unperceived in the technical sense of the word. Morgan's phrase, "a bit of pure experience," is perhaps as good as any. The loop is to the cat what the ocean is to a man, when thrown into it when half-asleep. Thus the cat who climbed up the front of the cage whenever I said, "I must feed those cats," would climb up just as inevitably when I said, "My name is Thorndike," or "To-day is Tuesday." So cats would claw at the loop or button when the door was open. So cats would paw at the place where a loop had been, though none was there. The reaction is not to a well-discriminated object, but to a vague situation, and any element of the situation may arouse the reaction. The whole situation in the case of man is speedily resolved into elements; the particular elements are held in focus, and the non-essential is systematically kept out of mind. In the animal the whole situation sets loose the impulse; all of its elements, including the non-essentials, get yoked with the impulse, and the situation may be added to or subtracted from without destroying the association, provided you leave something which will set off the impulse. The animal does not think one is like the other, nor does it, as is so often said, mistake one for the other. It does not think *about* it at all; it just thinks *it*, and the *it* is the kind of "pure experience" we have been describing. In human mental life we have accurate, discriminated sensations and perceptions, realized as such, and general notions, also realized as such. Now, what the phenomena in animals which we have been considering show is that they have neither. Far from showing an advanced stage of mentality, they show a very primitive and unspecialized stage. They are to be explained not by the presence of *general* notions, but by the absence of notions of *particulars*. The idea that animals react to a particular and absolutely defined and realized sense-impression, and that a similar reaction to a sense-impression which varies from the first proves an association by similarity, is a myth. We shall see later how an animal does come in certain cases to discriminate, in one sense of the word, with a great degree of delicacy, but we shall also see then what must be emphasized now, that naturally the animal's brain reacts very coarsely to sense-impressions, and that the animal does not think *about* his thoughts at all.

This puts a new face upon the question of the origin and development of human abstractions and consequent general ideas. It has been commonly supposed that animals had 'recepts' or such semi-abstractions as Morgan's 'predominants,' and that by associating with these arbitrary and permanent signs, such as articulate sounds, one turned them into genuine ideas of qualities. Professor James has made the simple but brilliant criticism that all a recept really means is *a tendency to react in a certain way*. But I have tried to show that the fact that an animal reacts alike to a lot of things gives no reason to believe that it is conscious of their common quality and reacts to that consciousness, because the things it reacts to in the first place are not the hard-and-fast, well-defined 'things' of human life. What a 'recept' or 'predominant' really stands for is no thing which can be transformed into a notion of a quality by being labelled with a name. This easy solution of the problem of abstraction is impossible. A true idea of the problem itself is better than such a solution.

My statement of what has been the course of development along this line is derived from observations of animals' behavior and Professor James' theory of the nature of and presumable brain processes going with the abstractions and conceptions of human consciousness, but it is justified chiefly by its harmony with the view that conception, the faculty of having general notions, has been naturally selected by reason of its utility. The first thing is for an animal to learn to react alike only to things which resemble each other in the *essential* qualities. On an artificial, analytic basis, feelings of abstract qualities might grow out of reacting alike to objects similar in such a respect that the reaction would be useless or harmful. But in the actual struggle for existence, starting with the mammalian mind as we have found it, you will tend to get reactions to the *beneficial* similarities by selection from among these so-called mistakes, *before you get any general faculty of noticing similarities*. In order that this faculty of indifferent reaction to different things shall grow into the useful faculty of indifferent reaction to different things *which have all some quality that makes the reaction a fit one*, there must be a tremendous range of associations. For a lot of the similarities which are non-essential have to be stamped out, not by a power of feeling likeness, but by their failure to lead to pleasure. With such a wide range of associations we may get reactions on the one hand where impulses have been connected with one particular sense-impression because when connected with all others they had failed to give pleasure, and on the other hand reactions where an impulse has been connected with numerous different impressions possessing one common quality, and disconnected with all impressions, otherwise like these, which fail to have that one quality.

Combined with this multiplication of associations, there is, I think, an equally important factor, the loosening of the elements of an association from one another and from it as a whole. Probably the idea of the look of the loop or lever or thumb-latch never entered the mind of any one of my cats during the months that they were with me, except when the front end of the association containing it was excited by putting the cat into the box. In general, the unit of their consciousness, apart from impulses and emotions, is a whole association-series. Such soil cannot grow general ideas, for the ideas, so long as they never show themselves except for a particular practical business, will not be thought about or realized in their nature or connections. If enough associations are provided by a general curiosity, such as is seen among the monkeys, if the mental elements of the association are freed, isolated, felt by themselves, ***then*** a realization of the ideas, feelings of their similarity by transition from one to the other, feelings of qualities and of meanings, may gradually emerge. Language will be a factor in the isolation of the ideas and a help to their realization. But when anyone says that language has been the cause of the change from brute to man, when one talks as if ***nothing but it*** were needed turn animal consciousness into human, he is speaking as foolishly as one who should say that a proboscis added to a cow would make it an elephant.

This is all I have to say, in this connection, about association by similarity and conception, and with it is concluded our analysis of the nature of the association-process in animals. Before proceeding to treat of the delicacy, complexity, number and permanence of these associations, it seems worth while to attempt to describe graphically, not by analysis, the mental fact we have been studying, and also to connect our results with the previous theories of association.

One who has seen the phenomena so far described, who has watched the life of a cat or dog for a month or more under test conditions, gets, or fancies he gets, a fairly definite idea of what the intellectual life of a cat or dog feels like. It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts *about* them or memories of how they looked at other times, or æsthetic judgments about their beauty; one feels no *ideas* about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connections of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all.

This pictorial description may be supplemented by an account of some associations in human life which are learned in the same way as are animal associations; associations, therefore, where the process of formation is possibly homologous with that in animals. When a man learns to swim, to play tennis or billiards, or to juggle, the process is something like what happens when the cat learns to pull the string to get out of the box, provided, of course, we remove, in the man's case, all the accompanying mentality which is not directly concerned in learning the feat.[1] Like the latter, the former contains desire, sense-impression, impulse, act and possible representations. Like it, the former is learned gradually. Moreover, the associations concerned cannot be formed by imitation. One does not know how to dive just by seeing another man dive. You cannot form them from being put through them, though, of course, this helps indirectly, in a way that it does not with animals. One makes use of no feelings of a common element, no perceptions of similarity. The tennis player does not feel, "This ball coming at this angle and with this speed is similar in angle, though not in speed, to that other ball of an hour ago, therefore I will hit it in a similar way." He simply feels an impulse from the sense-impression. Finally, the elements of the associations are not isolated. No tennis-player's stream of thought is filled with free-floating representations of any of the tens of thousands of sense-impressions or movements he has seen and made on the tennis-court. Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind.

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[1] A man may learn to swim from the general feeling, "I want to be able to swim." While learning, he may think of this desire, of the difficulties of the motion, of the instruction given him, or of anything which may turn up in his mind. This is all extraneous and is not concerned in the acquisition of the association. Nothing like it, of course, goes on in the animal's mind. Imagine a man thrown into the water repeatedly, and gradually floundering to the shore in better and better style until finally, when thrown in, he swims off perfectly, and deprive the man of all extraneous feelings, and you have an approximate homologue of the process in animals. He feels discomfort, certain impules to flounder around, some of which are the right ones to move his body to the shore. The pleasure which follows stamps in these and gradually the proper movements are made immediately on feeling the sense-impression of surrounding water.

Thus, the associations in human life, which compare with the simple connections learned by animals, are associations involving connections between novel, complex, and often inconstant sense-impressions and impulses to acts similarly novel, complex and often inconstant. Man has the elements of most of his associations in isolated form, attended to separately, possessed as a permanent fund, recallable at will, and multifariously connected among themselves, but with these associations which we have mentioned, and with others like them, he deals as the animals deal with theirs. The process, in the man's mind, leaving out extraneous mental stuff, may be homologous to the association-process in animals. Of course, by assiduous attention to the elements of these associations, a man may isolate them, may thus get these associations to the same plane as the rest. But they pass through the stage we have described, even then, and with most men, stay there. The abstraction, the naming, etc., generally come from observers of the game or action, and concern things as felt by them, not by the participant.

Criticism of Previous Theories.

We may now look for a moment at what previous writers have said about the nature of association in animals. The complaint was made early in this book that all the statements had been exceedingly vague and of no value, except as retorts to the 'reason' school. In the course of the discussion I have tried to extricate from this vagueness definite statements about imitation, association of ideas, association by ideas. There is one more theory, more or less hidden in the vagueness; the theory that association in animals is the same as association in man, that the animal mind differs from the human mind only by the absence of reason and what it implies. Presumably, silence about what association is, means that it is the association which human psychology discusses. When the silence is broken, we get such utterances of this theory as the following:

"I think we may say then that the higher animals are able to proceed a long way in the formation and definition of highly complex constructs, analogous to but probably differing somewhat from those which we form ourselves. These constructs, moreover, through association with reconstructs or representations, link themselves in trains so that a sensation, or group of sensations, may suggest a series of reconstructs, or a series of remembered phenomena." (C. L. Morgan, 'Animal Life and Intelligence,' p. 341.)

"Lastly, before taking leave of the subject of the chapter, I am most anxious that it should not be thought that, in contending that intelligence is not reason, I wish in any way to disparage intelligence. Nine-tenths at least of the actions of average men are intelligent and not rational. Do we not all of us know hundreds of practical men who are in the highest degree intelligent, but in whom the rational, analytic faculty is but little developed? Is it any injustice to the brutes to contend that their inferences are of the same order as those of these excellent practical folk? In any case, no such injustice is intended; and if I deny them self-consciousness and reason I grant to the higher animals perceptions of marvellous acuteness and intelligent inferences of wonderful accuracy and precision—intelligent inferences in some cases, no doubt, more perfect even than those of man, who is often disturbed by many thoughts" (ibid., p. 376–377).

"Language and the analytic faculty it renders possible, differentiate man from the brute" (ibid., p. 374).

Here, as elsewhere, it should be remembered that Lloyd Morgan is not quoted because he is the worst offender or because he represents the opposite in general of what the present writer takes to be the truth. On the contrary, Morgan is quoted because he is the least offender, because he has taken the most advanced stand along the line of the present investigation, because my differences from him are in the line of his differences from other writers. With the theory of the passages just quoted, however, which attribute extensive association of ideas and general powers comparable to those of men minus reason, to the brutes, and which repeat the time-honored distinction by language, I do not, in the least, agree. Association in animals does not equal association in man. The latter is built over and permeated and transformed by inference and judgment and comparison; it includes imitation in our narrow sense of transferred association; it obtains where no impulse is included; it thus takes frequently the form of long trains of thought ending in no pleasure-giving act; its elements are often loose, existing independently of the particular association; the association is not only thought, but at the same time thought *about*. None of these statements may be truthfully made of animal association. Only a small part of human association is at all comparable to it. My opinion of what that small part is has already been given. Moreover, further differences will be found as we consider the data relating to the delicacy, complexity, number, and permanence of associations in animals. I said a while ago that man was no more an animal with language than an elephant was a cow with a proboscis. We may safely broaden the statement and say that ***man is not an animal plus reason***. It has been one great purpose of this investigation to show that even after leaving reason out of account, there are tremendous differences between man and the higher animals. The problem of comparative psychology is not only to get human reason from some lower faculties, but to get human *association* from animal association.

Our analysis, necessarily imperfect because the first attempted, of the nature of the association-process in animals is finished and we have now to speak of its limitations in respect to delicacy, complexity, number and permanence.

DELICACY OF ASSOCIATIONS.

It goes without saying that the possible delicacy of associations is conditioned by the delicacy of sense-powers. If an animal doesn't feel differently at seeing two objects, it cannot associate one with one reaction, the other with another. An equally obvious factor is attention; what is not attended to will not be associated. Beyond this there is no *a priori* reason why an animal should not react differently to things varying only by the most delicate difference, and I am inclined to think an animal could; that any two objects with a difference appreciable by sensation which are also able to win attention may be reacted to differently. Experiments to show this are very tedious and the practical question is, "What will the animal naturally attend to?" The difficulty, as all trainers say, is to get the animal's attention to your signal somehow. Then he will in time surely react differently, if you give him the chance, to a figure 7 on the blackboard from the way he does to a figure 8, to your question, "How many days are there in a week?" and to your question, "How many legs have you?" The chimpanzee in London that handed out 3, 4, 5, 6 or 7 straws at command was not thereby proved of remarkable intelligence or of remarkably delicate associative power. Any reputable animal trainer would be ashamed to exhibit a horse who could not do as much 'counting' as that. The maximum of delicacy in associating exhibited by any animal, to my knowledge, is displayed in the performance of the dog 'Dodgerfield,' exhibited by a Mr. Davis, who brings from four cards, numbered 1, 2, 3 and 4, whichever one his master shall *think of*. That is, you write out an arbitrary list, *e. g.*, 4, 2, 1, 3, 3, 2, 2, 1, 4, 2, etc., and hand it to Mr. Davis, who looks at the list, thinks of the first number, says "Attention! Dodger!" and then, "Bring it." This the dog does and so on through the list. Mr. Davis makes no signals which anyone sitting even right beside or in front of him can detect. Thus the dog exceeds the human observers in delicacy and associates each with a separate act four attitudes of his master, which to human observers seem all alike. Mr. Davis says he thinks the dog is a mind-reader. I think it quite possible that whatever signs the dog goes by are given unconsciously and consist only of some very delicate general differences in facial expression or the manner of saying the words, "Bring it," or slight sounds made by Mr. Davis in thinking to himself the words one or two or three or four. Mr. Davis keeps his eyes shut and his hands behind a newspaper. The dog looks directly at his face.

To such a height possible delicacy may attain, but possible delicacy is quite another thing from actual untrained and unstimulated delicacy. The difference in reaction has to be brought about by associating with pleasure the reaction to the different sense-impression when it itself differs and associating with pain tendencies to confuse the reactions. The animal does not naturally as a function of sense-powers discriminate at all delicately. Thus the cat who climbed up the wire netting when I said, "I must feed those cats!" did not have a delicate association of just that act with just those words. For after I had dropped the clapping part of the signal and simply used those words it would react just as vigorously to the words, "To-morrow is Tuesday" or "My name is Thorndike." The reaction naturally was to a very vague stimulus. Taking cat 10 when just beginning to learn to climb up at the signal, "I must feed those cats!" I started in to improve the delicacy, by opposing to this formula the formula, "I will not feed them," after saying which, I kept my word. That is, I gave sometimes the former signal and fed the cat, sometimes the latter and did not. The object was to see how long the cat would be in learning always to go up when I gave the first, never to do so when I gave the second signal. I said the words in both cases as I naturally would do, so that there was a difference in emphasis and tone as well as in the mere nature of the syllables. The two signals were given in all sorts of combinations so that there was no regularity in the recurrence of either which might aid the animal. The cat at first did not always climb up at the first signal and often *did* climb up at the wrong one. The change from this condition to one of perfect discrimination is shown in the accompanying curves, one showing the decrease in *failures* to respond to the right signal, the other showing the decrease in responses to the wrong signal. The first curve is formed by a line joining the tops of perpendiculars erected at intervals of 1 mm. along the abscissa. The height of a perpendicular represents the number of times the cat failed to respond to the food-signal in 20 trials, a height of 1 mm. being the representative of one failure. Thus, the entire curve stands for 280 trials, there being no failures after 60 trials, and only 1 after the 40th.

In the other curve, also, each 1. mm. along the abscissa stands for 20 trials, and the perpendiculars whose tops the curve unites represent the number of times the cat in each 20 *did* climb up at the signal which meant no food. It will be seen that 380 experiences were necessary before the animal learned that the second signal was different from the first. The experiment shows beautifully the animal method of acquisition. If at any stage the animal could have isolated the two ideas of the two sense-impressions, and felt them together in comparison, this long and tedious process would have been unnecessary.

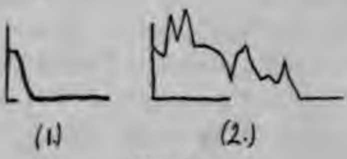

FIG. 20.

It might be stated here that the animals also acquired associations of moderate delicacy in discriminating between the different boxes. No cat tried to get out of A or B by licking herself, for instance.

The question may naturally be raised that if naturally associations are thus vague, the common phenomenon of a dog obeying his master's commands, and no one else's, is inexplicable. The difference between one man and another, one voice and another, it may be said, is not much of a difference, yet is here uniformly discriminated, although we cannot suppose any such systematic training to reject the other slightly differing commands. My cats did not so discriminate. If anyone else sat in my chair and called out, "I must feed the cats," they reacted, and probably very many animals would, if untroubled by emotions of curiosity or fear at the new individual, go through their tricks as well at another's voice as at that of their master. The other cases exemplify the influence of attention. Repeated attention to these sense-impressions has rendered them clear-cut and detailed, and the new impression consequently does not equal them in calling forth the reaction.

The main thing to carry away from this discussion is the assurance that the delicacy of the animal in associating acts with impressions is nothing like the delicacy of the man who feels that a certain tone is higher, or weight is heavier, than another, but *is* like the delicacy of the man who runs to a certain spot to hit one tennis ball and to a different spot to hit one coming with a slightly different speed.

COMPLEXITY OF ASSOCIATIONS.

An important question, especially if one wishes to rate an animal in a scale of intelligence, is the question of how complex an association it can form. A man can learn that to open a door he has to put the key in its hole, turn it, turn the knob, and pull the door. Here, then, is a complex act connected with the simple sense-impression. Or, conversely, a man knows that when the ringing of a bell is followed by a whistle and that by a red light he is to do a certain thing, while if any of the three happen alone he is not to. How far, then, we ask, can animals go along the line of increased complexity in the associations?

We must not mistake for a complex association a series of associations, where one sense-impression leads to an act such as to present a new sense-impression which leads to another act which in its turn leads to a new sense-impression. Of the formation of such *series* animals are capable to a very high degree. Chicks from 10 to 25 days old learned to go directly through a sort of big labyrinth requiring a series of 23 distinct and in some cases fairly difficult associations, of which 11 involved choices between two paths. By this power of acquiring a long series animals find their way to distant feeding grounds and back again. But all such cases are examples of the *number*, not of the complexity, of animal associations.

Some of my boxes were such as did give a chance for a complex association to be formed. Such were G (*thumb-latch*), J (*double*), K and L (triples) for the cats, and O (triple) for the dogs. It would be possible for a cat, after stepping on the platform in K, to notice that the platform was in a different position, and so feel then a different sense-impression from before, and thus turn the thing into a serial association. The cat would then be like a man who on seeing a door should feel only the impulse to stick the key in the hole, but then, seeing the door plus a key in the hole, should feel the impulse to turn the key and so on through. My cats did not give any signs of this, so that with them it was either a complex association or an irregular happening of the proper impulses. Probably the same was the case with dog 1. Cats 10, 11, 12 in L knew all the movements separately before being experimented on with the combination. Cats 2, 3, 4, had had some experience of D, which worked by a string something like the string part of K. The string in K was, however, quite differently situated and required an altogether different movement to pull it. Since further No. 2, who had had ten times as much experience in D as 3 or 4, succeeded no better with the string element of K than they, it is probable that the experience did not help very much. All else in all these compound associations was new. At the same time the history of these animals' dealings with these boxes would not fairly represent that of animals without general experience of clawing at all sorts of loose or shaky things in the inside of a box. These cats had learned to claw at all sorts of things. The time curves were taken as in the formation of the other associations, and, in addition, the order in which the animal did the several things required was recorded in every trial.

In the case of all the curves, except the latter part of 3 in G, one notices a very gradual slope and an excessive irregularity in the curve throughout. Within the limits of the trials given the animals are unable to form a perfect association and what advancement they make is very slow. The case of 3 in G is not an exception to this, but a proof of it. For 3 succeeded in making a perfect association, by accidentally hitting on a way to turn the compound association into a simple one. He happened one time to paw down the thumb-piece at the same time that his other fore-limb, with which he was holding on between the door and the top of the box, was pressing against the door. This giving him success he repeated it in later trials and in a short time had it fixed as an element in a perfect association. The marked change in his curve from an irregular and gradual slope at such a height as displayed a very imperfect association to a constant and very slight height shows precisely the change from a compound to a simple association.

Compound associations are formed slowly and not at all well. Further observation shows that they were really not formed at all. For the animals did not, except 3 in K for a certain period, do the several things in a constant order, nor did they do them only once apiece. On the contrary, an animal would pull the string several times after the bolt had gone up with its customary click, and would do sometimes one thing first, sometimes another. It may also be noted here, in advance of its proper place, that these compound associations are far below the simple in point of permanence. The conduct of the animals is clearly not that of minds having associated with a certain box's interior the idea of a succession of three movements. The animal does not feel, "I did this and that and that and got out," or, more simply still, "this and that and that means getting out." If it did we should soon see it doing what was necessary without repetition and in a fairly constant time.

I imagine, however, that an animal could learn to associate with one sense-impression a compound act so as to perform its elements in a regular order. By arranging the box so that the second and third elements of the act could be performed *only after the first had been*, and the third *only after the first and second*, I am inclined to think you could get a very vigorous cat to learn the elements in order and form the association perfectly. The case is comparable to that of delicacy. The cat does not *tend* to know what he is doing or to depart from the hit-or-miss method of learning, but by associating the other combinations of elements with failure to get pleasure, as in delicacy experiments we associated the reactions to all but the one signal, you could probably stamp out all but the 1, 2, 3 order.

The fact that you have to thus manœuvre to get the animals to have the three impulses in a regular order shows that even when they are so there is no idea of the three as in an order, no thinking about them. Representations do not get beyond their first intention. They are not carried up into a free life which works them over anew. A complex *act* does not imply a complex *thought*, or, more exactly, a performance of a series does not imply the thought of a series. Consequently, since the complexity of the act depends on the power which failure has to stamp out all other combinations, it is far more limited than in man.

NUMBER OF ASSOCIATIONS.

The patent and important fact is that there are so few in animals compared to the human stock. Even after taking into account the various acts associated with various smells, and exaggerating the possibility of getting an equipment of associations in this field which man lacks, one must recognize how far below man any animal is in respect to mere quantity of asssociations. The associations with words alone of an average American child of ten years far outnumber those of any dog. A good billiard-player probably has more associations in connection with this single pastime than a dog with his whole life's business. In the associations which are homologous with those of animals man outdoes them and adds an infinity of associations of a different sort. The primates would seem, by virtue of their incessant curiosity and addition to experience not for any practical purpose but merely for love of mental life, to represent an advanced stage toward this tremendous quantity of associations. In man not only this activity and curiosity, but also education, increases the number of associations. Associations are formed more quickly, and the absence of need for self-support during a long infancy gives time. Associations thus formed work back upon practical life, and by showing better ways decrease the need of work, and so again increase the chance to form associations. The result in the case of a human mind to-day is the possession of a thesaurus of valuable associations, if the time has been wisely spent. The free life of ideas, imitation, all the methods of communication, and the original accomplishments which we may include under the head of invention, make the process of acquisition in many cases quite a different one from the trial and error method of the animals, and in general much shorten it.

Small as it is, however, the number of associations which an animal may acquire is probably much larger than popularly supposed.

My cats and dogs did not mix up their acts with the wrong sense-impressions. The chicks that learned the series of twenty-three associations did not find it a task beyond their powers to retain them. Several three-day-old chicks, which I caused to learn ten simple associations in the same day, kept the things apart and on the next morning went through each act at the proper stimulus. In the hands of animal trainers some animals get a large number of associatious perfectly in hand. The horse Mascot is claimed to know the meaning of fifteen hundred signals! He certainly knows a great many, and such as are naturally difficult of acquisition. It would be an enlightening investigation if some one could find out just how many associations a cat or dog could form, if he were carefully and constantly given an opportunity. The result would probably show that the number was limited only by the amount of motive available and the time taken to acquire each. For there is probably nothing in their brain structure which limits the number of connections that can be formed, or would cause such connections, as they grew numerous, to become confused.

In their anxiety to credit animals with human powers, the psychologists have disregarded or belittled, perhaps, the possibilities of the strictly animal sort of association. They would think it more wonderful that a horse should respond differently to a lot of different numbers on the black-board than that he should infer a consequence from premises. But if it be made a direct question of pleasure or pain to an animal, he can associate any number of acts with different stimuli. Only he does not form any association until he has to, until the direct benefit is apparent, and, for his ordinary life, comparatively few are needed.

On the whole our judgment from a comparison of man's associations with the brutes' must be that man's are naturally far more delicate, complex and numerous, and that in as far as the animals attain delicacy, complexity, or a great number of associations, they do it by methods which man uses only in a very limited part of the field.

PERMANENCE OF ASSOCIATIONS.

Once formed, the connections by which, when an animal feels a certain sense-impression, he does a certain thing, persist over considerable intervals of time. With the curves on pages 18 to 26 and 33 to 34 are given in many instances[1] additional curves showing the animal's proficiency after an interval without experience. To these data may be added the following:

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[1] See 10 in A, 3 in A, 10 in D; 10 in C, 4 in C, 3 in C; 6, 2, 5, 4 in E; 4 in F; 10 in H, 3 in H; 3, 4, 5, in I; 4 in G, 3 in G; 3 in K; 10 in L; dog 1 in N and CC; dog 1 in G and O.

The three chicks that had learned to escape through the long labyrinth (involving twenty-three associations) succeeded in repeating the performance after ten days' interval. Similarly the chicks used as imitators in V, W, X and Y, did not fail to perform the proper act after an interval of twenty days. Cat 6, who had had about a hundred experiences in C *(Button)*, had the association as perfect after twenty days as when it left off. Cat 2, who had had 36 experiences with C and had attained a constant time of 8 sec., escaped fourteen days later in 3, 9 and 8 secs. respectively, in three trials. Cat 1, after an interval of twenty days, failed in 10 minutes to escape from C. The signal for climbing up the front of the cage was reacted to by No. 3 after an interval of twenty-four days. No. 10, who had learned to discriminate between 'I must feed those cats' and 'I will not feed them,' was tried after *eighty days*. It was given 50 trials with the second signal mingled indiscriminately with 25 trials with the first. I give the full record of these, 'yes' equalling a trial in which she 'forgot' and climbed up, 'no' equalling a trial in which she wisely stayed down. Dashes represent intervening trials with the first signal, *to which she always reacted*. It will be observed that 50 trials put the cat in the same position that 350 had done in her first experience, although in that first experience she had had only about a hundred trials after the association had been perfected. The association between the first signal and climbing up was perfect after the eighty days.

Trials 1-7.	Trials 8-17.	Trials 18-27.	Trials 28-35.	Trials 36-42.	Trials 43-50.
——	yes	no	——	——	——
——	yes	yes	——	no	——
yes	yes	no	——	no	——
yes	——	no	no	——	——
no	yes	——	no	no	——
——	yes	——	yes	no	no
yes	no	yes	no	no	no
yes	yes	yes	——	——	yes
no	no	yes	no	——	no
no	——	yes	yes	no	no
——	——	no	no	no	no
——	yes	no	no		no
——	yes				no
	——				

All these data show that traces of the connections once formed are very slow in being lost. If we allow that part of the time in the first trial in all these cases is due to the time taken to realize the situation (time not needed in the trials when the association is forming and the animal is constantly being dropped into boxes) we may say that the association is as firm as ever for a considerable time after practice at it is stopped. How long a time would be required to annul the influence of any given quantity of experience, say of an association which had been gone through with ten times, I cannot say. It could, if profitable, easily be determined in any case. The only case of total loss of the association (No. 1 in C) is so exceptional that I fancy something other than lapse of time was its cause. The main interest of these data, considered as quantitative estimates, is not psychological, but biological. They show what a tremendous advantage the well-developed association-process is to an animal. The ways to different feeding grounds, the actions of enemies, the appearance of noxious foods, are all connected permanently with the proper reaction by a few experiences which need be reinforced only very rarely. Of course, associations without any permanence would be useless, but the usefulness increases immensely with such a degree of permanence as these results witness. An interesting experiment from the biological point of view would be to see how infrequently an experience could occur and yet lead eventually to a perfect association. An experiment approximating this is recorded in the time-curves for Box H in Fig. 4, on page 20. Three trials at a time were given, the trials being two or three days apart. As may be seen from the curves, the association was readily formed.

The chief psychological interest of these data is that they show that permanence of associations *is not memory*. The fact that a cat, when after an interval she is put into box G, proceeds to immediately press the thumb-piece and push the door, does not at all mean that the cat feels the box to be the same from which she weeks ago freed herself by pushing down that thumb-piece, or thinks about ever having felt or done anything in that box. She does not refer the present situation to a situation of the past and realize that it is the same, but simply feels on being confronted with that situation the same impulse which she felt before. She does the thing now for just the same reason that she did it before, namely, because pleasure has connected that act above all others with that sense-impression, so that it is the one she feels like doing. Her condition is that of the swimmer who starts his summer season after a winter's deprivation. When he jumps off the pier and hits the water he swims, not because he remembers that this is the way he dealt with water last summer and so applies his remembrance to present use, but just because experience has taught him to feel like swimming when he hits the water. All talk about recognition and memory in animals, if it asserts the presence of anything more than this, is a gross mistake. For real memory is an absolute thing, including everything but forgetfulness. If the cat had real memory it would, when after an interval dropped into a box, remember that from this box it escaped by doing this or that and consequently, either immediately or after a time of recollection, go do it, or else it would not remember and would fail utterly to do it. On the contrary, we have all grades of *partial* 'forgetfulness,' just like the grades of swimming one might find if he dropped a dozen college professors into the mill-ponds of their boyhood, just like the grades of forgetfulness of the associations once acquired on the ball-field which are manifested when on the Fourth of July the 'solid men' of a town get out to amuse their fellow citizens. The animal makes attacks on a spot around the vital one, or claws at the thing—but not so precisely as before, or goes at it a while and then resorts to instinctive methods of getting out. Its actions are exactly what would be expected of an animal in whom the sense-impression aroused the impulse imperfectly, or weakly, or intermittently, but are not at all like the actions of one who felt, "I used to get out of this box by pulling that loop down." In fact, the record of No. 10 given on page 96 seems to be final on this point. If at any time in the course of the 50 trials it had *remembered* that 'I will not feed them' meant 'no fish,' it would thenceforth have failed to react. It would have stopped short in the 'yes' reactions, instead of gradually decreasing their percentage. 'Memory' in animals, if one still chooses to use the word, is *permanence of associations*, not the presence of an idea of an experience attributed to the past.

To this proposition two corollaries may be added. First, these phenomena of incomplete forgetfulness extend the evidence that animals do not have a stock of independent ideas, the return of which plus past associates equals memory. Second, there is, properly speaking, no continuity in their mental streams. The present thought does not clutch the past to its bosom or hold the future in its womb. The animal's self is not a being 'looking before and after,' but a direct practical association of feelings and impulses. So far as experiences come continuously they may be said to form a continuous mental life, but there is no continuity imposed from within. The feelings of its own body are always present and impresssions from outside may come as they come to us. When the habit of attending to the elements of its associations and raising them up into the life of free ideas is acquired, these permanent bodily associations may become the basis of a feeling of self-hood and the trains of ideas may be felt as a continuous life.

INHIBITION OF INSTINCTS BY HABIT.

One very important result of association remains to be considered, its inhibition of instincts and previous associations. An animal who has become habituated to getting out of a box by pulling a loop and opening the door will do so even though the hole in the top of the box be uncovered, whereas, if, in early trials, you had left any such hole, he would have taken the instinctive way and crawled through it. Instances of this sort of thing are well-nigh ubiquitous. It is a tremendous factor in animal life, and the strongest instincts may thus be annulled. The phenomenon has been already recognized in the literature of the subject, a convenient account being found in James' *Psychology*, Vol. II., pages 394–397. In addition to such accounts, one may note that the influence of association is exerted in two ways. The instinct may wane by not being used, because the animal forms the habit of meeting the situation in a different way, or it may be actually inhibited. An instance of the former sort is found in the history of a cat which learns to pull a loop and so escape from a box whose top is covered by a board nailed over it. If, after enough trials, you remove a piece of the board covering the box, the cat, when put in, will still pull the loop instead of crawling out through the opening thus made. But, at any time, if she happens to notice the hole, she *may* make use of it. An instance of the second sort is that of a chick which has been put on a box with a wire screen at its edge, preventing her from jumping directly down, as she would instinctively do, and forcing her to jump to another box on one side of it and thence down. In the experiments which I made, the chick was prevented by a second screen from jumping directly from the second box also. That is, if in the accompanying figure, A is a box 34 inches high, B a box 25 inches high, C a box 16 inches high, and D the pen with the food and other chicks, the subject had to go A–B–C–D. The chick tried at first to get through the screen, pecked at it and ran up and down along it, looking at the chicks below and seeking for a hole to get through. Finally it jumped to B and, after a similar process, to C. After enough trials it forms the habit and when put on A goes immediately to B, then to C and down. Now if, after 75 or 80 trials, you take away the screens, giving the chick a free chance to go to D from either A or B, and then put it on A, the following phenomenon appears. The chick goes up to the edge, looks over, walks up and down it for a while, still looking down at the chicks below, and then goes and jumps to B as habit has taught it to do. The same actions take place on B. No matter how clearly the chick sees the

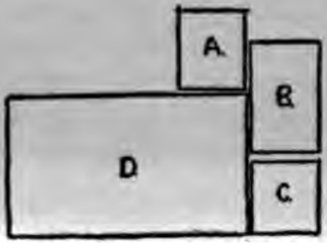

FIG. 21.

chance to jump to D, it does not do so. The impulse has been truly inhibited. It is not the mere habit of going the other way, but the impossibility of going *that* way. In one case I observed a chick in whom the instinct was all but, yet not quite inhibited. When tried without the screen, it went up to the edge to look over *nine times*, and at last, after seven minutes, did jump straight down.

ATTENTION.

I have presupposed throughout one function which it will be well to now recognize explicitly—attention. As usual, attention emphasizes and facilitates the process which it accompanies. Unless the sense-impression is focussed by attention, it will not be associated with the act which comes later. Unless two differing boxes are attended to, there will be no difference in the reactions to them. The really effective part of animal consciousness, then, as of human, is the part which is attended to; attention is the ruler of animal as well as human mind.

But in giving attention its deserts we need not forget that it is not here comparable to the whole of human attention. Our attention to the other player and the ball in a game of tennis *is* like the animal's attention, but our attention to a passage in Hegel, or the memory which flits through our mind, or the song we hear, or the player we idly watch, is *not*. There ought, I think, to be a separate name for attention when working for immediate practical associations. It is a different species from that which holds objects so that we may define them, think about them, remember them, etc., and the difference is, as our previous sentence shows, not that between voluntary and involuntary attention. The cat watching me for signs of my walking to the cage with fish is not in the condition of the man watching a ball game, but in that of the player watching the ball speeding toward him. There is a notable difference in the permanence of the impression. The man watching the game can remember just how that fly was hit and how the fielder ran for it, though he bestowed only a slight quantity of attention on the matter, while the fielder may attend to the utmost to the ball and yet not remember at all how it came or how he ran for it. The one sort of attention leads you to *think* about a thing, the other to *act* with reference to it. We must be careful to remember that when we say that the cat attended to what was said, we do not mean that he thereby established an idea of it. Animals are not proved to form separate ideas of sense-impressions because they attend to them, for the kind of attention they give is the kind which when given by men results in practical associations, not in establishing ideas of objects. If attention rendered clear the idea, we should not have the phenomena of incomplete forgetfulness lately mentioned. The animal would get a definite idea of just the exact thing done and would do it or nothing. The human development of attention is in closest connection with the acquisition of a stock of free ideas.

Social Consciousness.

Besides attention there is another topic somewhat apart from our general one, which yet deserves a few words. It concerns animals' social consciousness, their consciousnes of the feelings of their fellows. Do animals, for example, when they see others feeding, feel that the others are feeling pleasure? Do they, when they fight, feel that the other feels pain? So level-headed a thinker as Lloyd Morgan has said that they do, but the conduct of my animals would seem to show that they did not. For it has given us good reason to suppose that they do not possess *any* stock of isolated ideas, much less any abstracted, inferred or transferred ideas. These ideas of others' feelings imply a power to transfer states felt in oneself to another and realize them as there. Now it seems that any ability to thus transfer and realize an idea ought to carry with it an ability to form a transferred association, to imitate. If the animal realizes the mental states of the other animal who before his eyes pulls the string, goes out through the door and eats fish, he ought to form the association, 'impulse to pull string, pleasure of eating fish.' This we saw the animal could not do.

In fact, pleasure in another, pain in another, is not a sense-presentation or a representation or feeling of an object of any sort, but rather a 'meaning,' a feeling '*of the fact that.*' It can exist only as something thought *about*. It is never 'a bit of direct experience,' but an abstraction from our own life referred to that of another.

I fancy that these feelings of others' feelings may be connected pretty closely with imitation, and for that reason may begin to appear in the monkeys. There we have some fair evidence for their presence in the tricks which monkeys play on each other. Such feelings seem the natural explanation of the apparently useless tail-pullings and such like which make up the attractions of the monkey-cage. These may, however, be instinctive forms of play-activity or merely examples of the general tendency of the monkeys to fool with everything.

INTERACTION.

I hope it will not be thought impertinent if from the standpoint of this research I add a word about a general psychological problem, the problem of interaction. I have spoken all along of the connection between the situation and a certain impulse and act being stamped in when pleasure results from the act and stamped out when it doesn't. In this fact, which is undeniable, lies a problem which Lloyd Morgan has frequently emphasized. *How are pleasurable results able to burn in and render predominant the association which led to them?* This is perhaps the greatest problem of both human and animal psychology. Unfortunately in human psychology it has been all tangled up with the problems of free will, mental activity, voluntary attention, the creation of novel acts, and almost everything else. In our experiments we get the data which give rise to the problem, in a very elementary form.

It should first be noted about the *fact* that the pleasure does not burn in an impulse and act themselves, but an impulse and act *as connected with that particular situation.* No cat ever goes around clawing, clawing, clawing all the time, because clawing in these boxes has resulted in pleasure. Secondly, the connection thus stamped in is *not contemporaneous, but prior to* the pleasure. So much for the fact; now for the explanation. I do not wish to rehearse or add to the arguments with which so many pages have been already filled by scientists and philosophers both. What we need most is not argument, but accurate accounts of the mental fact and of the brain-process. But I do wish to say to the parallelist, what has not to my knowledge been said, that if he presupposes, to account for this fact, a 'physical analogue of the hedonic consciousness,' it is his bounden duty to first show how any motion in any neuron or group of neurons in the nervous system can possess this power of stamping in any current which causes it. For no one would, from our present knowledge of the brain, judge *a priori* that any motion in any part of it could be conceived which should be thus regnant over all the others. And next he must show the possibility of the current which represents the association being the excitant of this regnant motion in a manner direct enough for the purpose.

I wish also to say that whoever thinks that, going along with the current which parallels the association, there is an accompanying minor current, which parallels the pleasure and which stamps in the first current when present with it, flies directly in the face of the facts. *There is no pleasure along with the association. The pleasure does not come until after the association is done and gone.* It is caused by no such minor current, but by the excitation of peripheral sense-organs when freedom from confinement is realized or food is secured. Of course, the notion of such a secondary sub-current is mythology, any way.

To the interactionist I would say: "Do not any more repeat in tiresome fashion that consciousness *does* alter movement, but get to work and show when, where, in what forms and to what degrees it does so. Then, even if it turns out to have been a physical parallel that did the work, you will, at least, have the credit of attaining the best knowledge about the results and their conditions, even though you misnamed the factor."

Besides this contribution to general psychology, I think we may safely offer one to pedagogical science. At least some of our results possess considerable pedagogical interest. The fundamental form of intellection, the association-process in animals, is one, we decided, which requires the personal experience of the animal in all its elements. The association cannot be taught by putting the animal through it or giving it a chance to imitate. Now every observant teacher realizes how often the cleverest explanation and the best models for imitation fail. Yet often, in such cases, a pupil, if somehow enticed to do the thing, even without comprehension of what it means, even without any real knowledge of what he is doing, will finally get hold of it. So, also, in very many kinds of knowledge, the pupil who does anything from imitation, or who does anything from being put through it, fails to get a real and permanent mastery of the thing. I am sure that with a certain type of mind the only way to teach fractions in algebra, for example, is to get the pupil to do, do, do. I am inclined to think that in many individuals certain things cannot be learned save by actual performance. And I think it is often a fair question when explanation, imitation and actual performance are all possible methods, which is the best. We are here alongside the foundations of mental life, and this hitherto unsuspected law of animal mind may prevail in human mind to an extent hitherto unknown. The best way with children may often be in the pompous words of an animal trainer, 'to arrange everything in connection with the trick so that the animal will be compelled by the laws of his own nature to perform it.'

This does not at all imply that I think, as a present school of scientists seem to, that because a certain thing *has been* in phylogeny we ought to repeat it in ontogeny. Heaven knows that Dame Nature herself in ontogeny abbreviates and skips and distorts the order of the appearance of organs and functions, and for the best of reasons. We ought to make an effort, as she does, to omit the useless and antiquated and get to the best and most useful as soon as possible; we ought to change what *is* to what *ought to be*, as far as we can. And I would not advocate this animal-like method of learning in place of the later ones unless it does the same work better. I simply suggest that in many cases where at present its use is never dreamed of, it may be a good method. As the fundamental form of intellection every student of *theoretical* pedagogy ought to take it into account.

There is one more contribution, this time to anthropology. If the method of trial and error, with accidental success, be the method of acquiring associations among the animals, the slow progress of primitive man, the long time between stone age and iron age, for instance, becomes suggestive. Primitive man probably acquired knowledge by just this process, aided possibly by imitation. At any rate, progress was not by seeing through things, but by accidentally hitting upon them. Very possibly an investigation of the history of primitive man and of the present life of savages in the light of the results of this research might bring out old facts in a new and profitable way.

Comparative psychology has, in the light of this research, two tasks of prime importance. One is to study the passage of the child-mind from a life of immediately practical associations to the life of free ideas; the other is to find out how far the anthropoid primates advance toward a similar passage, and to ascertain accurately what faint beginnings or preparations for such an advance the early mammalian stock may be supposed to have had. In this latter connection I think it will be of the utmost importance to bear in mind the possibility that *the present anthropoid primates may be mentally degenerate.* Their present aimless activity and incessant, but largely useless, curiosity may be the degenerated vestiges of such a well-directed activity and useful curiosity as led *homo sapiens* to important practical discoveries, such as the use of tools, the art of making fire, etc. It is even a remote possibility that their chattering is a *relic* of something like language, not a *beginning* of such. Comparative psychology should use the phenomena of the monkey-mind of to-day to find out what the primitive mind from which man's sprung off was like. That is the important thing to get at, and the question whether the present monkey-mind has not gone back instead of ahead is an all-important question. A natural and perhaps sufficient cause of degeneracy would be arboreal habits. The animal that found a means of survival in his muscles might well lose the means before furnished by his brain.

To these disconnected remarks still another must be added, addressed this time to the anecdote school. Some member of it who has chanced to read this may feel like saying: "This experimental work is all very well. Your cats and dogs represent, it is true, specimens from the top stratum of animal intelligence, and your negations, based on their conduct, may be authoritative so far as concerns the average, typical mammalian mind. But our anecdotes do not claim to be stories of the conduct of the average or type, but of those exceptional individuals who have begun to attain higher powers. And, if even a few dogs and cats have these higher powers, our contention is, in a modified form, upheld." To all this I agree, provided the anecdote school now realize just what sort of a position they hold. They are clearly in pretty much the same position as spiritualists. Their anecdotes are on pretty much the same level as the anecdotes of thought-transference, materializations of spirits, super-normal knowledge, etc. Not in quite the same position, for far greater care has been given by the Psychical Research Society to establishing the criteria of authenticity, to insuring good observation, to explaining by normal psychology all that can be so explained, in the case of the latter than the anecdote school has done in the case of the former. The off-hand explanation of certain anecdotes by invoking reason, or imitation, or recognition, or feelings of qualities, is on a par with the explanation of trance-phenomena and such like by invoking the spirits of dead people. I do not deny that we may get lawfully a super-normal psychology, or that the super-normal acts it finds may turn out to be explained by these functions which I have denied to the normal animal mind. But I must soberly declare that I think there is less likelihood that such functions are the explanation of animal acts than that the existence of the spirits of dead people is the true explanation of the automatisms of spiritualistic phenomena. So much for the anecdote school, if it calls itself by its right name and pretends only to give an *abnormal* animal psychology. The sad fact has been that it has always pushed forward these exceptions as the essential phenomena of animal mind. It has built up a general psychology from abnormal data. It is like an anatomy written from observations on dime-museum freaks.

CONCLUSION.

I do not think it is advisable here, at the close of this paper, to give a summary of its results. The paper itself is really only such a summary with the most important evidence, for the extent of territory covered and the need of brevity have prevented completeness in explanation or illustration. If the reader cares here, at the end, to have the broadest possible statement of our conclusions and will take the pains to supply the right meaning, we might say that our work has described a method, crude but promising, and has made the beginning of an exact estimate of just what associations, simple and compound, an animal can form, how quickly he forms them, and how long he retains them. It has described the method of formation, and, on the condition that our subjects were representative, has rejected reason, comparison or inference, perception of similarity, and imitation. It has denied the existence in animal consciousness of any important stock of free ideas or impulses, and so has denied that animal association is homologous with the association of human psychology. It has homologized it with a certain limited form of human association. It has proposed, as necessary steps in the evolution of human faculty, a vast increase in the number of associations, signs of which appear in the primates, and a freeing of the elements thereof into independent existence. It has given us an increased insight into various mental processes. It has convinced the writer, if not the reader, that the old speculations about what an animal could do, what it thought, and how what it thought grew into what human beings think, were a long way from the truth, and *not on the road to it.*

Finally, I wish to say that, although the changes proposed in the conception of mental development have been suggested somewhat fragmentarily and in various connections, that has not been done because I think them unimportant. On the contrary, I think them of the utmost importance. I believe that our best service has been to show that animal intellection is made up of a lot of specific connections, whose elements are restricted to them, and which subserve practical ends *directly*, and to homologize it with the intellection involved in such human associations as regulate the conduct of a man playing tennis. The fundamental phenomenon which I find presented in animal consciousness is one which can harden into inherited connections and reflexes, on the one hand, and thus connect naturally with a host of the phenomena of animal life; on the other hand, it emphasizes the fact that our mental life has grown up as a mediation between stimulus and reaction. The old view of human consciousness is that it is built up out of elementary sensations, that very minute bits of consciousness come first and gradually get built up into the complex web. It looks for the beginnings of consciousness to *little* feelings. This our view abolishes and declares that the progress is not from little and simple to big and complicated, but from direct connections to indirect connections in which a stock of isolated elements plays a part, is from 'pure experience' or undifferentiated feelings, to discrimination, on the one hand, to generalizations, abstractions, on the other. If, as seems probable, the primates display a vast increase of associations, and a stock of free-swimming ideas, our view gives to the line of descent a meaning which it never could have so long as the question was the vague one of more or less 'intelligence.' It will, I hope, when supported by an investigation of the mental life of the primates and of the period in child life when these directly practical associations become overgrown by a rapid luxuriance of free ideas, show us the real history of the origin of human faculty. It turns out apparently that a modest study of the facts of association in animals has given us a working hypothesis for a comparative psychology.

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BY

GEORGE VAN NESS DEARBORN, A.M., M.D.

Sometime Assistant in Philosophy in Harvard University.

[Submitted in partial fulfilment of the requirements for the degree of Doctor of osophy in the Faculty of Philosophy, Columbia University, and being Vol. IV., No. 4, olumbia University Contributions to Philosophy, Psychology and Education.]

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THE EMOTION OF JOY.

It is the main purpose of this essay to outline a description of the Emotion of Joy in both its aspects, psychical and physical. On the one hand we shall try to set forth what the emotion 'feels like,' and on the other to point out the chief concomitant bodily movements, strains and postures and to explain, so far as may be, by what biological principles these are what they are seen to be.

The concept which should be perhaps suggested to the serious scientist more often than any other by the work he does is the concept of complexity. However numerous and comprehensive, generalizations always fail to keep pace with the infinite detail which the senses of the seeker offer to him. Through description, then, of smaller and ever smaller portions of subject matter, it would seem that some compensation might be obtained for this discrepancy. Thus it is that one emotion, out of all the indefinite many all men know, is at present a proper division of the affective field. Each 'emotion' is a mode or habit of action by itself, and one by one each needs explanation and description founded on basal principles possibly common to them all. But before taking up the discussion of the single emotion selected as the subject of this research, we shall express certain considerations on the nature and process of emotion in general, thus making more definite our meaning as regards joy.

Only one variation in the affective terminology which is perhaps the most common will be employed in this essay, and that is one which the kinæsthetic theory of emotion almost necessitates if it is to be scientifically set forth. The term 'expression' as applied to the bodily movements of emotion has not only lost its force, but is distinctly misleading. For my own use, therefore, the more fitting term *extramotion* has been devised, correlative to which *intramotion*, referring to the mental or sensational side of affective phenomena, will be employed. Inasmuch as the body-movements, in part at least, are perceptible from without and their concomitant quantums and quales of consciousness experienceable only as it were from within, these terms have a certain logical basis, while it is important that the complete correspondence of the two orders of events should have some sort of expression in the terms employed in discussing them. Above all, is it essential to avoid such words as imply the primacy or superiority of either the psychical or the physical orders of emotional events. In this essay, then, intramotion may be taken to stand for the consciousness factor and extramotion for the somatic factors of movement and tendencies to movement. Should the term 'expression' be employed, as may at times happen for convenience sake, it should be understood in the sense now sufficiently indicated.

In this condensed essay any proper discussion of feeling as such can have no place; a page or two will suffice in which to set forth the notion of it here presupposed, while its more practical implications follow.

Only experience of them can, of course, convey the meaning of the feelings. Theoretically, however, in its essential nature feeling seems to be roughly definable as the reaction of personality upon the sensations which constitute its psychological basis. It is owing to the complexity of the self, therefore, that there are in organic experience such a multitude of feelings and that these are interwoven into such a complicated mass of sentiency. As chief part always in any feeling, analysis discriminates bodily sensations, correlate to the action of the many sense-organs connecting the subject with its physical environment. Second, there is a constant ideal element, derived from the memory-mass of cognitions and of affective 'traces.' It is this element of feeling which chiefly differentiates in consciousness feeling from mere sensation as abstracted by the psychologist. It seems to be the relative proportions of these two factors which give the extraordinary variety to the feelings, the one regularly decreasing as the other becomes more evident in consciousness. The ideal element indicates an awareness of the meaning, causes and mechanism of the feeling experienced, thus ministering to the understanding, while the sensational element or elements express to the subject the immediate aspect of the self as reacting upon its surroundings. To a naïve lover of fine music, for example, the sensational elements furnish pleasure and little technical knowledge of the art of the composer and the players, but should this auditor become a technical musician, the intellectual factor would increase at the expense of the agreeable feeling-tone. A third element of feeling comes from the psychophysical habits of any subject as expressive of the life-purpose of that self. It is this last factor which makes each person's feelings so different from those of every other, while the reaction and interaction between the various combinations of these habits help to increase to the greatest intricacy and variety the feelings within the experience of any subject.

However important to the doctrine of emotion and allied states of consciousness pleasantness and unpleasantness may be, we shall in this essay have little or nothing to do with pleasure and pain as such. Indeed, it is a presupposition of this present description that this discrimination is an important one for the scientific clearing up of the nature of affective states. Leaving to histological neurology the ultimate decision as to the existence of organs (peripheral, central and intermediary) belonging to pleasure and pain, we shall get along very well for all the indecision, being confident that emotion gets its affective tone, whether agreeable or the contrary, from organic conditions far more general than any that could be concerned in the neural structures rendered probable by the work described by Dr. H. Nichols and others. This distinction seems one of the most basal that can be made in this direction, for whether the physical basis of the discrimination be or be not discovered and proved in the nerve-fibers and nerve-cells, every person capable of a degree of introspection must know an absolute difference in kind between unpleasantness and pain as between pleasantness and pleasure properly so called. The intricacy of the primitive fibrillæ of nerve tissues maintained by

Max Schultze, Apáthy, Bethe and many other competent scientists makes the proper physical basis for the distinction as experience proves it, whether end-organs and the rest exist or not. Pleasantness and unpleasantness, on the other hand, seem much rather derivatives from certain nutritive conditions of the organism and from the self's reaction to conditions of a psychic nature attending its furtherance and hindrance than conditions of any sort dependent immediately on neural functioning. It is these feelings which alone are concerned in the nature of emotion; they are an innumerable multitude of states of consciousness, and include those classed as æsthetic, moral, social and the rest, a mass unnamed as they are unnamable in their closely interrelated confusion. It is with these, each with its large substratum of agreeableness and disagreeableness, that the theory of emotion is concerned, and never, save by accidental coincidence, with pleasure or pain proper—with the pains which more or less cut and bore into the soul or with the pleasures which are concerned with the primal and powerful biologic needs of life. Too little all through psychology, and especially in 'esthetology,' have the biologic principles of the psychophysical subject been considered. It is as it was and still is in part with Medicine: the abstraction of aspects needful to a complete and detailed science has caused too much attention to be given the special aspects, while all the time the proper object of research is the *total organism of mind and body*, the inheritance of myriad generations of development and evolution—a unity ever, in a sense, yet dependent always on a larger unity, and everywhere and always inevitably complex with biologic determinants pervading its every function. Such is the organism of whose life emotion is a part. It seems as unscientific to consider emotion unbiologically as to treat it as a purely psychic process, and into neither of these errors shall we fall, however inadequate such an essay as this may be to discuss properly the biologic field of affective states in any organism whatever, and most of all in man.

The so-called kinæsthetic theory is the foundation of the conception of emotion here to be discribed. Indeed, it is considered that this doctrine, as yet in its details and preciseness of statement relatively new, is the only one which can be harmonized with the modern trend of biology, evolutionary and 'monistic.' Only the broad curves of this plea of William James and Lange have yet been drawn; the details giving it completeness the years must gradually and laboriously supply.

Emotion is but a form of psychophysical experience abstracted for scientific convenience from out the empirical continuum of a living organism. The two sides run along together and states melt insensibly into each other. We cannot claim to hold that extreme degree of inassurance as to a definition of emotion which, for example, Professor Irons expresses when he in a recent article calls emotion unanalyzable and irreducible, an ultimate and primary aspect of mind; for feeling, proper, such a characterization would answer better. Here, almost more than elsewhere, introspection is of value, not perhaps at the precise time of the emotional experience, but just afterward, when its strongly affective intramotion and memory of its lively extramotion are both, perhaps, fully recallable and describable.

Again, it is apparent that observation is important and remunerative in the case of emotion because of the latter's essential bodily aspects, to be plainly seen, in part, in other organisms both brute and human. The psychologist himself may be, to be sure, of that calm and unruffled habit of mind that is proverbially the nature of the true philosopher, but his friends, his maidservant (very likely), butcher, cook, even his opponents in psychological discussion, not less than his house-cat and saddle-horse and the wilder acquaintances of his summer haunts, are sure to display emotions which he can study with no little advantage.

Emotion, then, appears in general to be a temporal abstraction from the continuum of our sentient experience, voluminous in its affective tone rather than acute, filling for the time the whole field of consciousness and that in a peculiarly 'extensive' degree. As emphatic expressions of the biologic purpose of the individual emotions are peculiarly inclusive of the whole consciousness for reasons which we shall later on attempt to explain. On the other aspect, namely the bodily, emotions are also as extensive as possible, recent physiological science making it probable that in a literal sense every portion of an organism supplied with nerves receives and partakes in the reverberation of an emotional event, undergoing both material and psychic change.

Emotions empirically have an indefinite number of variants in the two directions of quality and quantity. Besides the different emotions themselves, so numerous and interrelated that no one as yet has seriously attempted even to make a list of them, there are an indefinite number of qualities possible to each, while each empirical shade of each emotion may be felt in a further indefinite number of degrees of intensity. These facts are axiomatic—but unexplained. Where is one to search then for the determinants of all these affective states ?

The psychologue who does not as yet think his or her psychology in parallelistic terms would answer this question by referring simply to the free activity of mind as an independent principle, or else fall into the confusion which the halfway mode of belief sooner or later necessitates. As we have already said in substance, it is the hypothesis upon which this essay rests that, metaphysics aside, mind and body act in some kind of unison, however incognizable this may appear upon a cursory comparison of experienced feelings and contemporaneous bodily states for any given period. Such being the case, it is interesting to inquire how the states of mind are accompanied by states of the body. There is no pretense of having solved in any sense this immensely complicated problem, but even the roughest outline is here not out of place.

Just as the psychic side of an emotion is a total 'block' of consciousness more or less complete at any moment, so, and more completely so in a way, is the body a physiological unit, as already has been suggested. As it has done harm, howbeit perhaps necessary harm, to mental science to consider its 'faculties' and its 'ideas' as independent entities and *realen*, so physiology and even anatomy have at times failed to gain their best results because they have abstracted too much and kept too close to the methods of the scalpel and the microscope. Parts belong together, work together, and are parts only because a whole allows of their existence and common functioning. This is as true in the description of the emotions as it is elsewhere. A whole of psychic experience occupying, say, a minute of time is correlated by a like period of bodily strains and movements. But the psychic aspect of this complex event has innumerable possibilities of both quality and quantity—what bodily conditions correspond? We can be confident of answering the question philosophically if we say, The whole body with all its complexity corresponds, but its parts in very various degrees. A volume might in time suggest something of the detail here implied; such a volume would have interest, and the physiological psychology of the future will surely write it!

One would need to search in particular among the nerves, the bloodvessels, the muscles, the joints and the glands for the correlates of the intramotions of emotion. In nerves we include the brain and its ramifications so infinite in number and so omnipresent that they represent almost exactly the form and contour of the whole body, as a certain foreign masterpiece of dissection and maceration has demonstrated. By bloodvessels one includes the heart and its tubes, with the lymphatic system, nearly as universal in the body as the nerves. The muscles too are a unified mechanical system ranging in location from the upper fibers of the Occipito-frontalis to the Plantaris, and in size from the Stapedius to the Vastus externus. The joints are both numerous and very various, and especially rich in organs of sense. The glands, finally, have a part in affective correlation too often neglected, although they include such masses of nerve-accompanied cells, tingling with physiologic and chemic life, as the liver, alimentary system, testes, and mammæ, besides numerous other small but important glands scattered about the tissues. And from all and between all these and various many other busy structures of the higher animal frame, nerve fibers extend, making of their undescribed manifoldness a unity. It is to some such material structure as this that every emotional intramotion is concomitant—to a growth with its every cell alive and warm and significant with energy. These functioning structures vary, as the emotions do, both quantitatively and qualitatively, and both indefinitely.

As to the ***nervous system***, what these variations are in either mode is still undetermined, but they are doubtless most important. Very likely the correlations of difference which are the most essential arise in the various combinations of neural elements which obtain, in the mode of their association. This is Wundt's way of viewing the varieties and shades of emotion, and it has a certainty as far as it goes. But the brain is not the only neural organ whose action is concomitant in emotion; indeed it seems proper to think of the brain as a sort of switchboard of the varied movements and strains of the body. As to its intramotive aspect, the relations between feeling and the neural processes are much more intimate and what the Materialists (if any remain) would call causal. The importance of the very numerous ganglia scattered throughout the body must not be underestimated, for they seem to be essentially little brains placed conveniently for the more certain control over or service of the organs in or near which they lie. The nerve trunks, again, doubtless have something to do directly with the variations of the emotions.

The states of the *vascular system* are scarcely less various than those of the nerves. The blood-pressure varies within very wide limits, determined both by the heart rate and by the tone of a double set of muscular fibers in the arterial walls, as well as by the temporary state of the general nutrition. The relation between these variants and the neural variants is yearly becoming better known, as is its meaning for the individuality of the agent.

The feelings which are involved in the movements of hundreds of *muscles* and joints, each very rich in nervous instruments, and pressing ever variously on nerves, bloodvessels and glands, one would expect would be unclassifiable and unanalyzable, as experience proves them. It is only necessary for one to scowl and attempt to feel good-natured without unruffling his brow to learn how important are these tissues of infinite cells as determinants and variants of emotion. Under these circumstances it is possible to force into the fovea of consciousness thoughts which represent good nature, but in nowise, for the writer at least, an *affective* tone of amiability while the muscles scowl. This fact is no proof that practice, as by an actor, could not train the muscles to act independently of any feeling whatever, though it is certain that this is rarely the method of the greatest histrionic artists. For example, to Mrs. Siddons, who made great a success of imitating real affective positions and movements as few have done since, her acting was her womanhood and her womanhood her work. It is very probable that a census taken of actors and actresses whose art is of the highest success would give a strongly predominant proportion in the same direction. Art can do much, but that art does most and best which follows the methods of Nature. However perfect the apparent mechanism and the French art without, the astounding chess-playing automaton always has a real man within.

Recent researches[1] with the aid of the Röntgen rays on the movements of the stomach of the cat during the digestion of a meal showed in a most striking manner, that any slight vexation of the animal stopped with surprising promptness and invariability gastric contractions and movements of every sort. Thus is proved what many a man has suspected to occur in his own epigastrium in attacks of indigestion from some slight worry or feeling of haste.

The *joints* are only the hinges on which the bones turn upon each other, but they are portions of the body very well supplied with nerves most essential in the instinctive and voluntary movements of the frame. The joint and the muscular sensations combined form a mass of high emotional value, a combination however with a cœnesthetic timbre not further reducible by analysis.

But again, the *glands* give to consciousness (if this expression may for convenience be allowed, the materialistic implication being disavowed) a set of elements which in some emotions are of the very highest moment. The action of the large glands of digestion, liver, pancreas, etc., stops promptly on the accession of a shock of grief or of anger; tears both of grief and of joy are everyday phenomena, as are the diarrhœa of nervousness or of fear and the sweating and other glandular activity of amorousness and of extreme terror.

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[1] W. B. Cannon, *Am. Jl. Physiology*, May, 1898, Vol. I., No. III.

As a formula for scientific use a definition is of interest, but in case of a function so complex as emotion the construction of one, comprehensive yet simple, offers no little difficulty. This might be suspected from the greatly various attempts which have been made by mental scientists. In the first place, disagreement as to the nature of emotion is extreme, and dispute as to its proper natural limitations makes the matter worse. When the meaning of the word psychology itself is taken with so various values by its devotees, this disagreement is not surprising. As has been already suggested, emotion is the most complex of the aspects with which the psychologist has to do, and we should hardly expect that a definition would not correspond. It is not enough to say it is just this and that without qualifying or enlarging, for emotion seems to be no one simple somewhat but rather a complex of many. It will be convenient to express here, then, in as concise words as possible the present conception of an emotion, to be enlarged upon and explained later on:

Emotion may be said to be *a temporal portion of excited sentient experience wherein the subjectivity and the psychophysical attention to the object, real or ideal, are heightened with or without a tone of pleasantness or of unpleasantness, and wherein the feeling and the bodily position or movement are, or tend to be, characteristic and correlative.* Such a descriptive definition of emotion does not attempt to make known the meaning of an affective state to one who has never experienced it (although, of course, such a sentient being does not in fact exist), but it may well serve, at least, as a brief syllabus for future elaboration as well as a general résumé of observations and experiments.

Inasmuch as the last of the considerations of this formula will doubtless seem to the opponents of the so-called kinæsthetic theory (who, although decreasing, are still to be found) the part most in need of evidential confirmation, it may be proper to suggest certain propositions which certainly strengthen its general validity as a working descriptive hypothesis.

The relations existing between the extramotion and the intramotion of emotion have been the stumbling-block of unbelievers and the key-note of most of the objections which they have raised against the theory. The chief of these facts regarding this relation is undoubtedly the double circumstance, first, that in some cases at least the artificial production of bodily movements apparently like those of an extramotion gives in consciousness no proper emotion, while, second, that one often experiences emotion, in the sense of affective consciousness, without any obvious attending extramotion. To offer suggestions which would explain away this apparent discrepancy would be to make the kinæsthetic theory, in the present advanced stage of psychological physiology, acceptable to practically all modern-minded psychologists and physiologists. A beginning at this is attempted here, although the evidence in favor of the theory from recent researches in physiology as a science interconnected everywhere with psychology; from almost universal introspection by men; from observation on the lower animals; from actors who strive to base thoroughly imitative performances on introspective study of their method; from study of children under natural conditions; from careful introspective opinions of eminent and conservative psychologists in America and Europe; and from the natural presupposition of its truth; has grown into so convincing a mass of demonstration that there arises a proper demand for an explanation of the apparent facts that seem to tell against it. Most important by far of these apparent facts are the two, really but aspects of one circumstance, cited above.

It is not to be expected that any considerable number of psychologists will ever accept a dictum that emotion is nothing more nor less than the joint and muscle sensations which arise when the body moves in whole or in part. Stated in this bald way, as it was once unfortunately (perhaps for emphasis), the statement is undoubtedly untrue, because grossly incomplete; emotion is not so simple nor so easily explained and described. But this is the basis from which objections on the first score have been oftenest raised. Emotion, as analysis finds it, is indeed much more complex. For this purpose one must especially bear in mind the component which we have suggested as self-reference, or subjectivity, and that other, not less and perhaps more essential, namely, excitement. In a mere muscular imitation of an extramotion for experimental purposes neither of these is present in any proper sense in which it should be understood. Especially is there then no involvement of the biologic egotism of the subject; the aims and purposes, the honor, the satisfaction, the safety of the ego are not touched, are in no wise concerned. The movements artificially made have no proper meaning for the subject and are inevitably different on this account. Nothing has happened to stir up the cellular interest, so to say, of the agent in any such sense as emotion of necessity stirs it up. No suggestion is offered him that his ego is then in such relation with an object, real or ideal, that its feelings must be involved. In no way are the conditions parallel save in that the experimental and artificial movements cause sensations which presumably or theoretically are like those of emotion. This is on the presumption that the movements are imitated exactly, which is a doubtful supposition for any deliberate movement whatever. But any artificial imitation of an extramotion can by no means adequately represent the far-reaching and innumerable movements which are correlate with emotion: it is as if one should compare the basal theme of Tannhauser grandly played on a great organ with the sound of its air drummed with one finger upon one of the keyboards. Many of the muscles, perhaps most, innervated in an extramotion are wholly beyond the will's control. But more important than this, the larger mass of the feelings making up the substance of an intramotion are concomitant also to various movements other than muscular, and perhaps far within the body; vascular functions and glandular, secretive and excretive, of whose effects and innervation the agent is never conscious in any degree as disparate movements but which combine their multitudinous factors to make up the whole of the affective experience. From such considerations as these it is at once obvious that the conditions of mere cold experimental imitation of an extramotion and those of a natural emotion, vital and tingling with every phase and element of organic excitement, are wholly different, of necessity. Imitation of the conditions of the body in emotion is wholly impossible.

As regards the second part of the argument against the kinæsthetic theory, namely, that 'emotions' are often experienced when no obvious bodily movements occur at all (and this is the more valid of the two, by far), more complicated and far-reaching considerations have to be suggested, involving wider truths.

In the first place, the considerations as to the nature of the extramotion just now mentioned apply to this objection in a still more emphatic way. The bodily concomitants are not so simple and obvious to view as the objectors seem to suppose. The underlying and perhaps most essential elements of the extramotion are not visible to external view at all, but consist in a general and exceedingly complex stimulation of all, more or less, of the organic functions. Even in the case of the action of the muscles which are attached to the bones and move them, or which cause changes of place in considerable portions of the external tissues, at times easily visible, the fact that under affective conditions their contraction is not roughly visible to an on-looker is no proof whatever that the contraction does not in some measure occur. The manner in which the muscles are each enclosed in gliding fasciæ and packed about at times with fat allows of movements which from without are wholly invisible. A contraction, for example, of the facial muscles sufficient to entirely change the expression of the whole face may be brought about by molar changes so small as to be not at all definable, or only with great difficulty, by those most familiar with mimetic and the structural facts. It is only the grosser degrees of muscular contraction or relaxation which are apparent from a distance, while the vascular and glandular and internal muscular changes are not observable at all. It must be obvious that it is not that extensive muscular movement which bends the limbs or expands the chest to which a theory of emotion as a scientific doctrine properly refers, but that rather there is intended that theoretic sort of contraction which is indicated by the transmission of a motor impulse from the center to the contractile organ, which might respond visibly or not. Considerations homologous to this are pertinent for every sort of physiological unit concerned in an emotion, but they need not be set forth here.

It is continually obvious and it is one of the notable affective principles that cultured adults, among whom we need postulate no lack of emotive experiences, oftentimes exhibit little sign of emotion, and that, too, at times when it is reasonable to be sure that their consciousness is of a strongly affective tone. This fact does not apply to the lower animals, to savages, nor to naïve civilized children save in proportion to their culture, but yet the objection under discussion has been made from observation on the least natural class of humanity, the cultured adults. This objection may be met as follows, however inadequately set forth the facts may be.

The likeness of emotion, psychophysically, to attention, a somewhat less complex scientific abstraction, will be apparent to all who compare them, and the similarity has been noted in several descriptions of affective states. This likeness is a fundamental and far-reaching one, as may be seen from a definition of attention; it is fairly statable as follows: Attention is a state of psychophysical strain pointed by interest in certain directions for the purpose of aiding clearness of consciousness, and experienced as a cœnesthesia of bodily movements inhibitory or active; it is discrete in occurrence and either reflex or voluntary. Emotion is a sort of attention to an object either external or ideal, and it has as an essential component a complicated set, indeed a set of sets, of bodily movements or tendencies to movement, which act, however, always reflexly and run a more or less constant course throughout, both in space and in time. The chief characteristic of attention which is of interest to us here, however, is the fact that the process in question is mainly of a nature describable as *inhibitory*. So far in psychology and in physiology, research into the character of inhibition as a somewhat for scientific discussion has been wholly inadequate to its importance. Something has been accomplished as to the interference of percepts with each other, but that much larger and more important aspect of the subject which concerns the muscles and other organs and innervation in general has been neglected, explanatory facts herewith implicated being far to seek, and it is this mode of inhibition with which emotion and attention are in the main concerned.

It should be borne in mind that the notion inhibition does not of necessity imply that the process is in any sense a negative process, save as the grosser movements of various sorts as seen from without are concerned; in the sense that motion of this kind does not occur while in states other than inhibitory it might occur, it is a negative conception. But, as already pointed out, movement of this manifest sort is not the whole effect following on innervation of centrifugal nerves—strains may be produced, or even conflicts of neural energy which give no peripheral sign to perceiving ejects at all. Inhibition seems to mean practically the non-action which obtains contrary to what the instinct or habit would ordinarily occasion under more or less different conditions. From such a point of view the manifestations of inhibition are both numerous and important. All of the conditions in which the conscious focus passes rapidly from one object to another, such as hysteria, delirium, mania, seem to be states in which there is a deficiency of outer inhibition, while the opposed case, habits of repression, thought, voluntary action, delusions, ecstasy, melancholia, arise from a relative excess of repression from consciousness of the environment except the object attended to. The extramotion of emotion is in a proper sense a reflex attention to an instinctive, because vastly long inherited, series of bodily habits—habits based on principles which form the very groundwork of organic function.

Thus emotion is a complex sort of attention to one series of feelings and movements in the body, other considerations being by this circumstance excluded. But just in proportion as these other considerations, extrinsic to the emotion, enter the habitual series of neural events (whether consciously or unconsciously from the effect of early habitual repetition it matters nothing), the usual, theoretically complete, extramotion is changed or lessened or, apparently, from an external and cursory viewing, wholly suppressed. There is then inhibition of the instinctive motor ideas of the extramotion, a retention of these ingrained 'ideas' out of the usual reflex arc. It is pertinent to conjecture (and it is scientific in lack of proved facts as to the neural currents,) that in this process the nervous energy which in a wild animal, savage, or quite naïve child would have gone into the motor centers for the natural extramotion, will now be expended quite as completely, but through different neural paths and centers, perhaps resulting in a motionless balance of the bodily parts concerned, but certainly, in most cases at least, in such a way as to enliven the intramotion and make it often more intense.

> "The still, unuttered, silent, wordless grief
> That evermore doth ache, and ache, and ache,
> This is the sorrow wherewith hearts do break." (E. W. W.)

This indeed is a notion so very common in literature and speech everywhere that no psychologist can properly doubt its truth or avoid explaining it save on the presupposition that the psychologist alone knows human nature and the poet and the æsthete not at all. But be that as it may, the parallelistic doctrine of the relation of soul and body gets no little of its strength from its unconcern with the physical principle of the conservation of energy. It, therefore, is a gratuitous supposition that the energy involved in the two cases in comparison is equal, and it becomes a matter of empirical fact in the light of biological principles.

It has been a mistake of various treatments of the nature of emotion that the theory of evolution has not ordinarily been assumed as underlying its phenomena; thus must be considered every discussion of affection which draws conclusions from events in men alone, not deeming them animals at the apex of an infinite pyramid of others, but as loci in themselves sufficient for the adequate study of emotion's nature. Many a scientist still is loth to think of himself as the descendant of the past, failing to discriminate the criteria whereby man is man from those whereby he yet is brute. It surely is not the adult of the highest culture which our period knows who can best furnish us with an exhibition of emotions for our study; he is of all organisms we know the furthest removed from the average nature of animal function, and the study of his affective states can but lead astray in theorizing, however necessary it may be in practice to use him alone as subject in the laboratory. Many complex conditions peculiar to man, chiefly *social*, have placed him in a higher class apart, and proportionally *out* of the biologic series fading in the past behind him.

The proper standard of the extramotion of any emotion is to be obtained from study of the whole scale, more or less, of the animal kingdom, of absolute savages, and of infants not yet come under the influences of civilization proper. Many animals, of course, as well as relative savages and most children, have lost their biologic integrity in varying degrees, and display emotions, therefore, in proportional grades of imperfection.

Taking, then, as the well-understood though relatively undescribed set of extramotions that typical series of physical and chemical actions to be observed in organisms, of sufficient degree of evolution, retaining their organic simplicity, let us consider some of the conditions which in the highly artificial order of our civilized life tend to suppress the extramotions in very numerous cases, so giving rise to unwarranted objections as to the affective relations of body and soul.

Pursuant to the conditions of civilization and in particular of man's necessary struggle for existence, an intricate system of restraints and artificial restrictions has been gradually and inevitably developed by many centuries, how many no man dare say, of constantly acting motives leading to continually deeper-fixed modes of willing and conduct. Many of these motives for inhibition have brought about habits which are in effect instincts, and so numerous are these that in civilized lands it is uncommon to find any emotion expressed in the case of adults in that perfection of naturalness which elsewhere and among wild animals regularly obtains. In the infant the inhibitory process is begun regularly in its earliest months, and continues, either by deliberate instruction or by example or else imitatively, through life, none escaping wholly and few in any considerable degree from the all-mastering force of this advantageous restraint of once-natural bodily functions. Even the domestic animals display something of this universal influence, and one must go to the jungle or the bush to see what may be properly termed a natural extramotion. As remarks Gabriel Compayré in a recent book[1]: "Il y á presque toujours disproportion entre les faculteés expressives et les impressions ressenties; mais tandis que dans l'age mür, l'expression reste au-dessous de la realité, dans l'enface elle la dépasse," the latter consideration being a consequence of the undeveloped faculties of the very young child. The inhibiting motives are in reality complex and interinvolved to a degree proportionate to the social intricacies from which they have arisen; we can, however, suggest a few of those which act directly to restrain such of the emotional expressions as would be manifestly harmful to some degree to their subject, and some of these are here denoted, as applicable to ten of the commonest and most pronounced and well-defined of emotions.

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[1] 'L'evolution intellectuelle et morale de l'enfant,' 2d ed., p. 110.

MOTIVES FOR EXTRAMOTIVE RESTRAINT.

Fear.—Desire to be thought brave. Disadvantage of displaying fear to adversary. Better power of defense through muscular and other bodily control.

Anger.—Know personal and social advantages of peace. Habitual politeness. Cowardice. Policy. Sympathy.

Love.—Modesty. Prudery. Coyness. Honor.

Grief.—Fear of ridicule. Fear of pity. Modesty. Resignation. Vanity (wrinkles). Policy. Pride.

Hate.—Advantages of peace. Policy. Politeness. Benevolence. Cowardice. Self-respect.

Shame.—Pride. Arrogance.

Pride.—Fear of ridicule. Policy. Politeness.

Surprise.—Policy. Politeness. Pride.

Contempt.—Cowardice. Policy. Pride. Sympathy.

Joy.—Dignity. False politeness. Modesty. Policy. From reflection, by contrast, on grief and pain. 'Other-worldliness.' Pessimism. Vanity.

We will now leave the consideration of emotion in general and devote ourselves more particularly to Joy, although the whole of what has been thus far said applies toward a description of this emotion.

It were idle to set down, even for system's sake, in this abridged monograph, a detailed account of what a period of joy feels like to its subject-agent, for, as the fundamental emotion of all life, all persons are familiar, in some degree at least, with its appetible nature. It is enough to say that it forms the ideal experience to all naturally-minded mortals, and, eternalized, is the heaven most men look for. Immediate and retrospective introspection, however, makes easy an analysis of joy, as of other emotions, which has descriptive interest. The components suggested by this analysis have already been in the main intimated in the definitional formula of emotion as such given above, and need to be only somewhat enlarged upon as regards their details in the case of joy.

We distinguish five components in the state of delight we are considering. These are (1) Psychophysical excitement; (2) Various feelings and their concomitant bodily movements and strains; (3) Heightened consciousness of the emotion's object as in relation with the subject-agent; (4) A pleasant tone of consciousness, and (5) Increased self-reference. Under these five heads apparently the aspects of any emotion may be ranged, the appropriate changes being made in the fourth for the emotion considered. These are not separate in any actual sense, but are abstracted from the totality of the experience-period for study, as are often the clangs in a chord. The spirit of joyfulness all men know; let us glance at its components for psychology.

1. The psychophysical excitement of joy is the quantitative aspect, or intensity, of the emotion. This may vary within limits which are far apart, from a mere slight increase in the interest which the emotion's object has for the agent to a degree of excitement making all things else unperceived and the feelings themselves so intense as to reach a form of ecstasy or rapture in which the self is lost. Ordinarily, however, the excitement of joy is within such bounds that the mental and the bodily aspects may keep their proper ratio.

2. The mass or substance, so to term it, of joy is made up of a fusion of feelings concomitant to more or less characteristic bodily movements and strains. These movements, psychical and physical, we need scarcely attempt to describe, for the reason that the former are inherently indescribable and that the latter will be more intelligently explainable later on when the underlying principles shall have been set forth. The feeling-mass, however, is properly characterized as a consciousness of

satisfaction, both with one's self and with as much of its environment as is at the time involved with it; thus joy is active happiness in its most complete form, perfect trust for the time in the goodness of life. Joy therefore is pleasant in a degree in which oftentimes mere pleasure is not, for it signifies a satisfied activity, while many pleasures are lessened and their perfection tarnished by an accompanying consciousness of self-dissatisfaction, thoughts of pleasure's fleetness, desire for greater pleasure, or what not. More specifically, the intramotion of joy is a feeling-fusion, a general sort of cœnesthesia varying indefinitely according to the bodily organs active in concomitance, and dependent on the reaction of many shades of temperament and mood upon the yet more numerous surrounding situations serving for the time as object to the emotion. Thus the possible combinations of feelings making up a joy may have elements from three sets of determinants each very numerous: bodily parts, temperamental states, and objective conditions. If, for example, the elements of the first are capable of combining into, say, twenty sets of joy-extramotions, the temperamental tone combinations forty, and objects or occasions say two hundred, we should have theoretically 20 times 40 times 200, or 80,000, sorts or shades of joy possible in the social consciousness of humanity! It is owing to the excess of the two latter of these components that in children and savages joy is so comparatively more frequent, the former exhibiting also an instinctive useful bodily activity which is in itself a source of pleasantness, and the stronger because of the undamaged perfection of functions and sensations.

3. The heightened consciousness of an objective situation as in relation with the agent is a component of joy which may be in origin either material or ideal. This abstracted factor needs not many words in this emotion, and particularly because here the object is less determinative of the reaction than in some other emotions, as for example love, wherein the object determines to a great extent the emotion's mode. It is the interest-forcing quality of the object which determines the degree of the joy, and their valuation to the subject. The objects of joy themselves admit of no classification, so numerous and various are they.

4. The tone of pleasantness of joy has sometimes been considered by psychologists as almost its sum and substance, in earlier centuries in particular pleasure being used as almost a synonym of delight, a usage still met with at times. We have maintained that the affective tone of an emotion, however, is rather an accidental than a fundamental aspect of it, some being very pleasant, some unpleasant, but undeniably some also of such an empirical nature that it is impossible to say which of them obtains in any case. In other words, some emotions have no appreciable pleasantness or unpleasantness whatever, and both they cannot have. But joy is assuredly not of this number, and the strongest aspect of it is pleasantness; it is the type indeed of the emotions of this algedonic class, and this pleasantness seems to determine directly its instinctive extramotion, as we shall show, making it what it is. The pleasantness arises from the satisfaction of desire; from consciousness of the furtherance of the subject's biologic egotism; from the activity as such, essential to the emotion; from that general state of well-being which is denoted vaguely by good nutritive conditions; and, negatively, from the absence from consciousness of worry or unpleasantness, a negative condition brought about by the exclusive attention elsewhere which the emotion practically is.

5. The last component of joy which is distinct, increased self-reference, is an element less conspicuous in joy than in some other emotions, but one which should properly be mentioned, however hard to define in the terms of physiological psychology. This is the egotistic basis against which the emotion's object is apperceived, the standard of the agent's reaction. Of the furtherance of its purposes the ego in emotion seems to be conscious empirically as well as rationally, and in being aware of the emotion's object, the self at the same time is conscious of itself, because in affective states the reactive activity makes up the substance of the emotion, in this case the joy.

We will now go on to the experimental portion of this research into the nature of our chosen emotion. We shall continue the description of the bodily aspect of joy in a way, but shall be particularly engaged in an attempt to determine on a biological or organic basis why events happen in pleasant emotion as they are known to happen. We shall look somewhat at the 'what' but mainly at the 'why' of joyful extramotion.

It was deemed to be of interest to determine experimentally what degree of uniformity in the extramotion of joy exists when the occasioning object is a constant with all the subjects studied. One may, of course, observe continually, if he look in suitable places, expressions of joyfulness, but he can derive from such observation scarcely any quantitative results or any free from the variation, misleading in its nature, which the very inconstant bodily movements necessitate. The following set of experiments was undertaken, then, under the precisely defined conditions which accuracy requires. The number of the subjects employed was fourteen, and they were, with two exceptions, students in the Harvard Psychological Laboratory; five of them were young women, and the ages of the fourteen ranged between nineteen and forty-five, two of the men being of the latter relative extreme; the ages of most lay between twenty-two and thirty years.

It was primarily necessary that the occasion or object of the joy to be studied should be one that would appeal to all subjects alike and with nearly equal force, and, at the same time, not be so far out of the line of their experience that the conditions should be wholly strange; again, various degrees of stimulus were important, that it might be observed how the emotion varied proportionally to it. No other sort of object seemed to serve these requirements so well as gifts of money; no very wealthy persons, so far as known, being among the subjects, the acquisition of money would be as great a source of joy to one as to another and considerable to all. The only cause of regret involved in the use of money as an occasion of delight was the necessity of the gifts being hypothetical gifts, and not of real cash. Imagination supplied this necessary deficiency, however, for it was the constant endeavor throughout the set of experiments to induce the subject in every case to make the hypothetical money seem as real as possible, by means of repeated auto-suggestion and suggestions from the experimenter acting in the same direction. The subject in each case attempted to consider the supposed conditions actual at the time of each experiment, and then, as it were, read out of his affective consciousness, or will, what he or she *felt* like doing, thus in varying degree putting himself or herself into the actual conditions of the real emotion. It was, in a sense, the reading aloud of felt innervations. It is believed that in the very complex circumstances of the social consciousness, which a human emotion in reality represents, this mode, or other means to suggestion, is the only method available for producing a laboratory emotion under conditions which can be recorded. Hypnotism, of course, would have given the maximum of perfection of a suggested emotion, but it seems inexcusable to employ this agency except for therapeutic purposes—never in the laboratory, or as often, at least, as these experiments demanded. The certainty that these reports of action under the various conditions represent what the person studied would do under real joy of like origin was often increased by the remembrance and comparison of actual conduct under like circumstances in the past experience of the subject. Every person experimented upon was entirely naïve as to the experiment when it was begun.

The method of experimentation in this series was, then, as follows: The subject being seated comfortably, he or she was carefully and emphatically instructed to make the conditions suggested seem as real as possible in each case, then, after a few moments' consideration, to tell concisely what he felt like doing or would probably do under the proposed conditions, the accession of certain considerable amounts of cash. In all cases the suppositions in the circumstances of the gift were such that considerations foreign to the stimulation of a pure joyful emotion were suppressed, that is, no opportunity was given for a conflicting emotion at the death of relatives from whom the money came, no occasion for worry over the possible conditions of its acceptance, nor any other conflicting or inhibiting circumstance, for each hypothetical case represented an absolute and unconditioned accession of so much cash immediately to the subject, without the possibility of doubt. These conditions being understood, one after another hypothetical gifts of ten dollars, one hundred dollars, one thousand dollars, ten thousand dollars, and one hundred-thousand dollars, respectively, were made to the subject. What then would he or she do under these various circumstances?

There follows a synopsis of the statements in answer to this question, the reported probable extramotions from each different amount being arranged together for convenience of comparison. Each of the fourteen subjects is designated by a letter; the first five are the young women.

Ten Dollars.—Subject *A*, smiles (unless alone) and better spirits. *B*, smiles, rapidly walk the room, put desk in order, look in the mirror, be less attentive to work for an hour or so, better-natured, more lively, mentally and physically. *C*, broad smiles, play on the piano, no study. *D*, smiles, run for sympathy, walk straighter, hold head higher. *E*, smiles, hilarity, singing, or whistling, better humor, reflection. *F*, walk straighter and faster, eyes brilliant, muscles tense, more cheerful than usual, and better natured for a day or so. *G*, smiles, talk to himself or to others more, thoracic thrill, playful attack on his room-mate, active walking, no study. *H*, laughter, talk. *I*, smiles, no joy unless use appeared for the money, train of thought, better humor an hour or so. *J*, smile, nervous walking, stand a little straighter, more talkative, reflective, heart-beat faster. *K*, humming or whistling, get away from the letter (enclosing the money), psychophysically more active than usual, better self-possession, more elastic step, work hard that day. *L*, smiles, restlessness shown in walking up and down, increased sociability, more talkative, laughter at a small joke, whistling, step elastic, faster, erecter, no fatigue, might ‘cut’ a lecture or two. *M*, hug the money, smile, walk faster, whistle or sing to self, feeling of power, sociability, jig perhaps, work harder that forenoon. *N*, walk faster, body more erect, more affable.

One Hundred Dollars.—*A*, communicative on this subject, thoughtful and light-hearted for some days. *B*, much as before: heart would beat fast, flush, might feel faintish, jump up, run down stairs, treat myself to novel-reading, less attentive to work for a day or so, might hug a familiar chum. *C*, hilarity, jumping, laughter, continual grinning, play Sousa’s marches very loudly on the piano, singing. *D*, run excitedly for sympathy to some one, laughter, embraces, smiles, couldn't study well, might enjoy a walk if an errand were ready. *E*, seclusion and reflection of a melancholic and unpleasant sort, little work for a day or less, none of the common expressions of joy. *F*, more serious in manner, joy being on a higher plane, better natured for a week at least, expression of joy at first less than in the preceding case. *G*, succession of smiles, more joy felt but its expression less, thoughtfulness, restlessness, inattention to studies. *H*, grinning, active talk, walk straighter, would inhibit all expression of emotion. *I*, it would induce a train of thoughts only. *J*, reflection, nervously walk around the room, incipient smiles now and then, more talkative, might romp with my children. *K*, smiles, eyes brighter than usual, vision brighter, pulse and respiration faster, might look in the mirror occasionally, would wish to get out doors, firmer tread, better self-control, less noise than in the preceding case, tendency to smiles, good natured, kinder to people about, apt to work harder. *L*, would seek some crony, playful pugnacity, walk fast, straighter, might celebrate, heart-beat faster, much general physical energy, benevolence, no work for a day or two. *M*, less hilarity, desire and search for sympathy, tendency to quit work. *N*, better spirits, all faculties, mental and physical, accelerated, no work for the rest of that day, meditation.

One Thousand Dollars.—*A*, meditation or thoughtfulness, seek solitude, repress emotional expression. *B*, would walk round and round room, seek sympathy from some one at once, heart wild, leave work, go to the theatre, vanity, bicycle ride, 'scorching' some perhaps, dance, over-confidential. *C*, would dance, face radiant, later an expression of importance, stand straighter than usual, &c., unusually good-humored and generous, more smiles than usual. *D*, expression as in last experiment, only now I might cry some. *E*, heart wild, melancholy attack: shut myself up in my room several hours with painful thoughts unconcerned with money, absorbed, might either walk up and down or sit quiet. *F*, walk out into the country fast, nervously, exhilaration, might read a novel but should not study, emotional thrill in thorax, more deferential than usual, politer, bearing confident. *G*, smiles, less hilarity, walk with lighter step, faster, and straighter in position, muscular rhythm, tendency to the thoracic thrill, physical or mechanical work instead of mental, might take a trip away, frequent subdued smiles. *H*, smiles, talk to self, take a walk, whistle, sing, walk straighter, no hilarity. *I*, light a cigarette, omit lectures for a day or two, heart faster in beating, thoughtfulness. *J*, heart-rate greater, nervous and physical excitability, livelier for a week, with more singing. *K*, hilarity, playfulness, great self-possession, walk out, or up and down room rapidly, dignifiedly, cut lectures, confidential. *L*, seek company and sympathy, propose celebration to friends, physical excitability greater than in last experiment, drop work or do it ill, might be boyishly hilarious, talk to self, better humor for a week. *M*, reflection, physical energy greater than ordinarily, seek society, hilarious perhaps, no work. *N*, quieter except might be more talkative, would seek sympathy.

Ten Thousand Dollars.—*A*, should tell 'everybody' about it, excited, talkative, communicative, not hilarious, feeling of responsibility. *B*, general trembling, flush, graveness, take a long walk alone, slowly at first, then faster, confidential with some friend, generous, later hilarious, ride horseback and walk erecter and more firmly in general. *C*, expression would be as in last experiment only it would last longer, some sense and mien of superiority, brief extravagance. *D*, excited but never hilarious. *E*, melancholy attack for a day or two, secluded for several following days, reaction to cheerfulness gradual and moderate. *F*, would jump up and 'holler,' low yells of delight, little laughter, pulse and respiration faster, take a walk by the sea or sail upon it fast, mien alternating between exhilaration and quiet joy, latter lasting the longer. *G*, smiles alternating with periods of seriousness, less bodily movement than in the last case, might take a walk if an errand was at hand. *H*, smiles, might deliberately break some furniture, write letters, seek the crowd and dissipation, smoke Havana cigars fast, wander about listlessly and aimlessly. *I*, pulse faster, smoke, might go to the gymnasium, smiles, hilarity expressed in imaginary fencing or other boyishness. *J*, would act much as in last case, but the mental and physical activity would be increased, romp with my children, might go to the grand opera. *K*, serious bearing, go out alone for a walk away from people, reflective mood, followed by excitement, rapid walking, seek society, dinner party, etc., kinder to an organ-grinder, more affable. *L*, would seek his family and society, more affable and sociable, couldn't stay alone, sleeplessness, expression of joy more violent than in last case. *M*, less hilarious than in the last experiment but more happy. *N*, disquiet shown by walking up and down slowly, less hilarity than in last experiment, no physical signs of excitement.

One Hundred-Thousand Dollars.—*A*, would take a quiet walk alone, too excited mentally for hilarity, sense of responsibility. *B*, might faint, then absent-mindedly walk up and down, sighs, hilarity might develop under sympathetic surroundings, erect bearing, self-possession, strength, energy, dramatic posings perhaps. *C*, expression as in last case, only with more dignity and restraint, air of responsibility. *D*, same as last case, seek sympathy, trepidation, no hilarity. *E*, same as in last experiment. *F*, same as last time, save that the excitement would be less and last longer, great degree of self-poise. (*G*, missing.) *H*, great hilarity, smashing all the furniture in the room, telegraph some friends and seek others, alertness, hire a fast span and drive excitement off. *I*, no hilarity, but less depression than in last experiment. *J*, expression as before in some respects, exaggerated in some, and generally lessened by sense of responsibility. *K*, physical excitement less than in last case, dignity preventing hilarity. *L*, quiet walk alone, no hilarity, less impulsiveness than in last case, hilarity might obtain later on. *M*, friskiness, intense hilarity, jumping, waltzing, whistling, power and sense of security shown by poise of head which would be held higher. *N*, would take a trip alone, no hilarity, walk straighter and faster, with head more erect.

From this portion of our research several results in the line of our study appear which are of interest, and also data of suggestive value to an investigation of temperament. Perhaps the most important fact called to mind by these series of joyful movements, and to be demonstrated by no method perhaps more simple or effective than the one employed, is that joy is emphatically a sthenic emotion. One subject only in marked degree, namely *E*, and two others, *A* and *I*, in much less degree, avowed habits indicative of a general inhibition of the usual extramotion; the first of these was dependent doubtless on conditions verging on the pathological, while the occasion of the exceptional reports of the two other cases was due to an habitual dignity unusual in degree and quite obvious on acquaintance.

The next thing which appears as a product of the experiments is strongly corroborative of the basal principle that joy in its physical aspect stands regularly for expansion and extension of the body, both as to its immediate general occupation of space, and as to its locomotion through space, these being signified by the unusually erect posture, high-holding of the head, etc., and by the rapid walking, sailing, riding or driving which were very general and constant in the answers of the fourteen subjects. The piano playing, singing, loud talking, whistling, etc., are, of course, manifestations which belong in the same logical category. There is, in short and notably, from these experiments an expansion or extension, in a most general sense, of the personality active with the emotion.

A third result interesting to our purpose is the course of the development of the motives for inhibition, with the accession successively of the larger amounts of hypothetical funds, due to (*a*) a feeling of responsibility, (*b*) a sense of dignity, and to (*c*) now and then a touch of pride. Wealth as a source of power is latent activity, which becomes conscious and so kinetic activity (because of the motor nature of ideas) as soon as the shock of its accession is overcome; it is thus not only a source of joy from the pleasantness and pleasure which its spending actually provides, but it acts similarly with an immediate effect at the time of acquisition, because of the firmly fixed knowledge which every person has of it as an occasion of enjoyment in the future. Thus, joy is the realization of desire, which is in turn a motor idea located in whatever bodily organ or organs are to be most directly benefited, in this case, as is most usual, the whole body. The personality has life-plans expressed in the motor ideas habitual to that self; joy is but the sense of furtherance of that plan, so giving to consciousness satisfaction and very likely pleasantness, and this in turn is correlate with that personal expansion or extension which, we shall later on show in detail, is primal in the extramotion of pleasant emotions. Thus, an increased field is by this biological condition offered to the increased power, personal or social, of the affected agent. Joy may properly be termed the basal emotion, because it is so intimately related with psychophysical activity in the interests of the individual's biologic purposes.

Another result of this set of experiments germane to our immediate subject was so universally taken for granted by the reagents that in their reports they never mentioned it, yet it is of importance for consideration here. This universal presupposition is that joy above all other emotions of man is a condition of excited *pleasantness;* it has a pleasant tone so marked, indeed, that it seemed to some of the older philosophers a mental state synonymous with pleasure itself. Says Sir Charles Bell, however:[1] "Joy is distinguishable from pleasure. It consists not so much in the sense of gratification as in the delight of the conviction that the long-expected pleasure is within our reach, and the lively anticipation of the enjoyment which is now decked out in its most favorite and alluring shape." In this emotion, no doubt or uncertainty of judgment existing as to its algedonic tone, joy stands unequivocally for pleasantness, just as it is often a derivative of pleasure, either present or in certain prospect. Thus all mankind and perhaps the higher brutes seek joy as foliage the sun, or at least that general complex of pleasant exhilaration, mental and bodily, empirically known to all men, for which joy is as proper a term as any other. If often persons are to be observed who exhibit an inhibition, more or less complete, of all joyous extramotion, we may well postulate that in these cases the will, long become habitual in its mode, finds greater personal satisfaction in the suppression of the bodily correlates for the greater benefits derived from the supposedly larger or more important or at least more desired ends.

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[1] Anatomy and Philosophy of Expression, p. 152.

We may safely say, in general, that any emotion, other things equal, is pleasant to its subject in proportion to the energy which it expends, whether it be used, as the primal instinct impels, in the bodily movements which we may fairly term typical of that emotion, or in the inhibition of these movements accompanied by the consequent frequently greater intensity of the psychic side of the experience. If an emotion has in it unpleasant or painful elements, logically accidental to the emotion as such, these may, of course, overcome the pleasantness inherent in the activity; such, for example, is the case with fear, the most unpleasant of the emotions, and, as Mosso says, 'a disease to be cured.' Here in proportion to the activity manifested by the agent the experience is less unpleasant, the fright which kills at times being of the sort of which we say 'he was rooted to the spot.' Thus it appears that in general emotion is pleasant in proportion to its all-round energizing when other conditions, vital to the sustenance of the feeling of personality, do not obtain to counterbalance the biologic psychophysical tendency. It will appear, therefore, that, although the extramotion in general in the emotions of a savage or of a child appear to be more extensive than those of a highly developed, that is artificialized, adult of the United States, still the general plane of the emotion of the uncivilized agent is somewhat lower, because his mental complexity is less and hence the probability of confliction within the psychic series is materially decreased. The emotions in the naïve are, then, apparently both purer and more intense, but of a biologic automaticity which the cultured adult has outgrown, whether for ill or good.

Out of the maze of psychologic description and doctrine of the agreeable and the disagreeable, one fact at least seems fairly well agreed upon, namely, that in general pleasure and pleasantness make for a furtherance and pain and unpleasantness for a hindrance of the vital functions of the ego experiencing them. This is assumed as the foundation principle of a biologic science of feeling, and, although with pleasure and pain as such we are not at all concerned in a discussion of emotion, but only with pleasantness and unpleasantness, the notion is of prime importance to the doctrine of joy. This furtherance, then, which agreeableness indicates to any animal means to the biologic egotism of that animal growth, development, enlargement, expansion, extension, increase of the influential sphere of that ego by whatever mode it may be brought about, and whether psychically or physically or, as usually needs be, by the co-operation of both these aspects of selfness. To the mind, furtherance means greater influence, deeper worth and import, fuller satisfaction of the personal life-plans. To the body, it means augmentation and extension, either literally and immediately through change in form or movement of the limbs and organs, or else through locomotion, thus affording still another kind of extension and of its efficient energy's increase.

A notion of this relation, of this concomitance between spiritual and bodily furtherance, has been in a more or less vague way not uncommon in the minds of men of all periods. It is said that in some ancient Stoic philosopher's writing may be found the statement that bodily extension corresponds to pleasure of the mind, and contraction to pain. Paola Mantegazza, in his late interesting and systematic 'Physiognomy and Expression,' quotes from a book entitled 'Physiognomical Cephalogy,' by Cornelio Ghiradelli, published in Bologna in 1672, saying that pleasure extends even "to zoöphytes and animate plants, like the oysters and sponges, which contract as an effect of pain and which dilate with joy to the point of opening," and thus from Honoratius Niquetius's 'Physiognomia Humana,' published at Lugduni in 1648: "Voluptatis primus et maxima propius effectus est dilatio cordis sanguine et spiritu ad exterioris partes copiose effuso, unde et nonnullus gaudio propter nimiam spiritum jacturum, mortuos esse legimus . . ." Darwin indicates by his second principle of emotional expression an antithesis between affective movements and strains which is tenable only when taken in this sense. In the work of the poets the idea is of frequent occurrence in one or another form more or less significant. Schneider drew attention to its zoölogical relations. Professor Muensterburg first made the notion, however, a part of physiological psychology in a brief article in which he calls attention to the principle and carries it out to its logical conclusions as regards feeling.

This concomitance between feeling of pleasantness and of unpleasantness as pure consciousness and the respective bodily concomitants of each of these two sorts of feeling may be viewed, of course, from two points of view. From the one there is given a series of chiefly muscular movements and strains and we can observe what modes of feeling accompany them when analyzed into their obvious varieties as extensor and flexor. From the other view-point we can study the effects which pleasantness and unpleasantness, as such, have upon the muscular movements again as extensor and flexor.

With the former of these two possible methods of research we shall have relatively little to do in this essay, and that notwithstanding that it is philosophic to consider that it is rather, biologically speaking, the muscular and other bodily reactions which are primary from the phylogenetic standpoint. These are instinctive and based on hereditary and imitative habits in every organism. In the complexes of any empirical emotion, however, it is not possible to introspect and to say that from the contraction of the extensor muscles alone comes the experienced pleasantness; other factors enter in and confuse the certainty of the analysis. But it is open to the consciousness of any person adequately introspective that contraction of the extensor muscles gives a greater degree of agreeableness to its subject than does contraction of the muscles anatomically classed as flexor. This opinion has been arrived at independently as the result of almost instinctive observation by many, and has been proven by the writer, both by a series of experiments conducted for the purpose and by many others made with other intention. For example, if it may be allowed to anticipate a little for the purpose here, out of 1,328 voluntary extensor and flexor movements of the forearm through an arc of about forty degrees, two-thirds of the extensor movements were greater than the norm set for imitation, while the flexor movements were as often greater than the norm as they were less, but no more so. Both sorts of movements were accompanied by a faint degree of pleasantness, but those which were extensor evidently gave the more. The conditions in this case were equally favorable to extension and to flexion. But more certain even than such evidence is that of each man for himself, who, by the simple experiment of comparing the consciousness of the two sorts of movement (arms, head, trunk and legs), may assure himself that extension is more pleasant than flexion. This great fact, simple as it is, adds an important presumption to the phylogenetic theory of affective states, for it proves that pleasantness inheres in the very muscles when moving or strained, and that aside from the agreeableness arising from the mere activity as such. Whether in vital history the muscular movements preceded the pleasantness and unpleasantness or the contrary it would be an unwarranted presumption to declare at present; indeed it would be unphilosophic perhaps to say that either one preceded the other, eternal concomitance being probably the teleologic fact.

But practically, as regards experimentation in the psychological laboratory, the balance is not so perfect, for it is impracticable to make artificially an extramotion of joy, for example, and then to analyze the muscular feelings, as has before been said. On the other hand, to apply to the subject experimented upon a 'stimulus' which is pleasant or unpleasant, observing then the muscular and other bodily reactions, is certainly working nature backwards in a sense, such stimulations being wretchedly inadequate as producers of organic feelings of any extensity, and withal antecedent when they normally are concomitant, or perhaps at times consequent. This is the chief of the limitations which laboratory conditions demand of a research into the nature of emotion in normal subjects.

This series of experiments was carried out in the Psychological Laboratory of Harvard University throughout the college years 1895–1896 and 1897–1898. About a third part of this portion of the research has already been published independently, as of interest in itself, the article being entitled 'Involuntary Motor Reactions to Pleasant and Unpleasant Stimuli,' in No. 5, Vol. IV. of *The Psychological Review*. This portion of the experimental work was done with the able coöperation of Mr. Frank N. Spindler, now of the Michigan State Normal College.

The first part of the research deals with the reactions of the hand and of the head in the opposed algedonic tones. These parts were chosen because they are the most mobile portions of the external members of the body, the fingers in particular having a motor sensitivity excelled only by that of the respiratory muscles and of the heart. This is an empirical fact quite in line with the principle that, other conditions being equal, the most mobile parts are those most used; furthermore, inertia would tend to make the smallest muscles the most mobile. The head gets its high degree of mobility from the former of these two considerations hardly less than from the unique perfection of its balance upon the axis, maintained by very many small and delicate muscular bundles. We should expect therefore *a priori*, that, if any part of the organism showed extreme sensibility to changes in the affective tone of pleasantness and unpleasantness, it would be the fingers and the head. Our first problem for solution is, Is the extenso-flexor balance of the muscles of the hand and neck disturbable by such tones of pleasantness and of unpleasantness as are applicable to the subject under laboratory conditions?

The mechanical plan for the direct registration of the possible extension and flexion of the bodily parts now to be studied was as follows: The subject was seated in a comfortable chair with arms. A tightly-fitting pasteboard cap was then placed on the head from the center of the top of which cap a strong silk thread extended, over an easy-running pulley to the end of the lever-arm of a Marey tambour. Because the antero-posterior movements of the head were found to be in some cases considerable, this arm was made about 25 cm. long. By a careful centering of the pulley in the circle of the head's movements, all record of the occasional lateral motions of the head was avoided, account of these being of very minor interest in this research. Pneumatic pressure transferred in the usual way the rise and fall of the tambour's arm to the pen of another Marey tambour, tracing on smoked paper on a rotating drum.

The apparatus applied to the left hand consisted of a bulb small enough to be fairly grasped in the closed fist. No little difficulty was experienced in finding a bulb without so much resistance to compression that the subject's constant attention was necessary to keep it in the state of partial compression necessary to secure record of the extensor movements of the fingers. But at last a bulb made of a soft sponge from which the middle portions had been cut, enclosed in thin rubber dam, was suggested, and this served as a most sensitive and, indeed, generally adaptable piece of apparatus, for sponges may be found or cut of any desired degree of resilience. The varying pressure of the hand enclosing this bulb was, as before, pneumatically carried to a receiving tambour and recorded on smoked cylinder-paper at the left of the tracing made by the head.

To register the reactions of the right hand a different form of apparatus was arranged. About the second and third fingers, selected as the most sensitive and powerful of the five, a comfortable ring of brass foil was fastened. This was attached directly to the lever-arm of a tambour and as close to its fulcrum as possible, that any movement might be emphasized, and it was adjusted so that when the fingers and hand were moderately flexed the tambour-head was plane. Perfect comfort of the hand was found to be of great importance here, in order that voluntary attention to it and its reactions might be the more successfully avoided. The flexor and extensor movements of the fingers were, as before, recorded on the cylinder, but this time at the right of those of the head.

The more easily to secure a constant pressure at the start in the three sets of apparatus, the open ends of branches from the three conducting tubes were arranged side by side convenient to the operator, and fitted with clips so as to be simultaneously closed when all was ready and the kymograph in regular motion. The speed of the recording cylinder was such that one rotation was made in about five minutes. The cylinder was 14 cm. in diameter and 25 cm. long, being large enough for two records such as these without change of paper. Straight 'normals' were regularly run round the drum for the better measurement of the record-curves.

A complete account of the various conditions of each experiment was written with a stylus on each sheet, including the subject's name, temperament, subjective experiences, the stimulus, nature of its affective effect (whether pleasant, the contrary, or neither), data and direction of the muscular movement in each reaction. The subjects were mostly seniors and juniors of Harvard College, of Radcliffe College and graduate students of philosophy working in the laboratory. Their number was nineteen. Inquiries as to emotional likes and dislikes were regularly made, and as to their musical and 'artistic' education.

The emotional stimuli mostly used were odors, although sounds and variously colored light were employed to a much less extent. It was eminently desirable in every case that the stimuli employed should be as purely pleasant and unpleasant, whichever it was, as possible; this demand odors seem to satisfy immediately and almost always well; again, many subjects were employed and very many experiments made on each, and odors were the most convenient in application. Indeed, it has seemed to the experimenter over and over that odors furnish about the only unequivocal means of affording a pleasant stimulation in a general university laboratory, especially in researches where a considerable number of subjects or 'reagents' are employed. Again, odors are unlimited in number, thus suiting all subjects sooner or later; and they are relatively free from irrevalent and undesirable associations. It was much more difficult to find for every subject a number of positively disagreeable odors than to find agreeable ones, students of chemistry being especially hard to suit with a sufficiently disagreeable smell. Constant care was needed and exercised to suit the tastes of the subjects in this regard, the object regularly being to employ as stimuli typical and emphatic examples of what was judged respectively agreeable and the reverse.

The particular olfactory stimuli employed were contained in ounce vails kept on a convenient stand made for them. It is hardly possible to make any classification of these odors as pleasant or unpleasant, so erratically do tastes differ in this affective field. Roughly, however, they may be arranged in the following order of agreeableness to the greater number of the particular young men and women employed; the most pleasant comes first, but the middle members of the list, of necessity, vary very largely in this respect. The list is as follows: Oil of bergamot, cologne water, heliotrope, methyl acetate, oil of cloves, musk, ethyl iodide, spirits of turpentine, xylol, eugenol, oil of eucalyptus, iodoform, cider vinegar, carbon bisulphide, ethyl borneol and camphor, sulphuric ether, toluidin, allyl alcohol, asafœtida, diamylamine, acetic acid, and ammonium valerianate. A few subjects avowed no strong unpleasantness from any of these, and for these ammonia was employed in place of a real odor. It will be observed that 'disgusts' were not included among the stimuli, associations being undesirable in these experiments, which were with pure affective tones.

Panes of glass about 30 cm. square, colored red, blue, green, and orange, were used for the subjects with the color-taste highly developed, the panes being held for a few seconds each before their eyes. Similarly, for subjects musically inclined, such sweet tones as tuning forks can give were applied as stimuli, with harsh noises for the contrary intent. These two forms of stimulus seemed to have only minor advantages over odors, one of these being that they tire the perceiving end-organ somewhat less quickly than odors do; but they were more pleasant to the subject on the average than were the disagreeable members of the olfactory series, thus to that extent serving less well, while being much less easily applied with the precision both in time and space which accuracy requires. All these considerations have important application throughout this whole set of experiments.

During the five minutes of each experimental period (this being the time required for one revolution of the record-cylinder) from three to seven stimuli would be applied, varying with the nature of the odor, whether lingering or not. Each time the subject indicated whether the stimulus was pleasant or unpleasant to him, and this judgment served as the basis for the counting of the record made. Each subject was instructed to sit quietly during the experiments, yet not strained nor stiff, making no voluntary movements whatever. It was interesting to note that only very exceptionally was there any awareness of the involuntary flexions and extensions which occurred in the hand and head, the eyes, save in the case where light was used as stimulus, being closed.

The numerical results of this portion of the experiments were as follows, summarized and explained:

UNDER PLEASANT STIMULATION.

Taking each movement or lack of movement, whether of the head or of either hand, as a separate case, there were recorded 500 effects of sensory stimuli which were judged pleasant by the subjects. Of these, 118, or 23%, were cases of flexion of the hands or forward (flexor) movement of the head; 134, or 27%, were cases of no reaction whatever, and 248, or 49.6%, were cases of extension of the fingers or of backward (extensor) movement of the head. Considering the actual reactions only, there occurred *68% of movements of extension and only 32% of flexions*—a proportion of more than two to one. The tendency under pleasantness is therefore proven strongly toward extension, so far as the human hand and head are concerned.

The two hands and the head did not necessarily act together in the same way. The left hand appears much more sensitive to involuntary reaction than the right, and this was to be expected perhaps, most of the subjects being right-handed and, therefore, with their right hands 'civilized,' so to say, away from the original biologic habits of emotional concomitance.

Counting the cases of pleasant stimuli when the left hand showed no reaction, there were for the left hand under stimuli judged agreeable these results: Flexion, 21%; no reaction, 19%, and extension, 60%. Out of 184 stimulations the left hand flexed 37 times; did not react 35 times, and extended 112 times. Comparing the left hand with the right, the percentage of 'no reactions' is seen to be much the less for the left hand, while that of both flexions and of extensions is greater. Under pleasant stimulus the right hand showed: Flexion, 20%; no reaction, 40%, and extension, 40%. That is, in a total of 130 cases, the right hand flexed 27 times, did not react in 51 instances, and extended 52 times. The left hand, then, was indifferent 19% while the right was indifferent 40%; it flexed 21% while the right flexed 20%; and it extended 60% while the right hand extended only 40%. Leaving out the cases of 'no reaction' *the right hand flexed 34% and extended 66%; but the left hand flexed 25% and extended in 75% of its opportunities.*

As for the *head* under pleasant stimulation, it was found to flex in 29% ; it did not react in 26% and it extended or drew back 45%. That is, in a total of 186 pleasant stimulations, the head bent forward 54 times; bent backwards 84 times; and did not react 48 times. Comparing the flexions with the extensions alone, the head under agreeable stimulation was flexed 39% of the possible times, and extended 61%. The head was, then, more 'indifferent' than the total average results, but more reactive than the right hand. It will be observed that the head displays more of a tendency to flex under pleasant stimuli than does either hand,—viz., 29% of flexions to 20% for the right hand and 21% for the left hand. This was one of the surprises of these experiments: in many cases under pleasant stimulation the head would flex even when both hands extended or relaxed. At first this was supposed to be true only in cases when odors were used as stimuli and because of the necessary mode of applying these to the nose, but it was found that the same fact obtained when colors and sounds were employed. This must be an adaptive movement, for often, after this flexor reaction, the head would extend during the continuance of the pleasant stimulation. We shall observe the same phenomenon in the legs later on.

UNDER UNPLEASANT STIMULATION.

If now the head movements in response to unpleasant stimulation be examined, similar differences of reaction will be seen. Under this circumstance it flexed 43%; gave no reaction 19%; and extended 38%, or in a total of 168 cases it moved forward 72 times; showed no movement 32 times; and inclined backwards 64 times. Taking flexion and extension alone, there was 53% of flexions compared with 47% of extensions. Even more marked here is the tendency for the head to extend under an unpleasant stimulus than there was for it to flex under a pleasant stimulus, while both hands flexed. Yet for the head also flexion predominates. Comparing the hands as to their reaction and sensitivity to unpleasant stimuli, we find the right hand here also markedly more inhibited (or 'indifferent') than the left, for the left hand shows flexion 66.7%; no reaction 14.5% and extension 18.7%—or in 165 cases it flexed 110 times; gave no reaction 24 times; and extended 31 times. Leaving out the cases of 'no reaction,' it gave 78% of flexions and only 22% of extensions.

The right hand, however, flexed only 49.5% of the times; while it failed to react 29%; and extended 21%. Comparing flexion and extension cases alone, the right hand flexed 69% and extended 31%. The right hand, therefore, exhibits more tendency to indifference and to extension under disagreeable stimulation than does the left hand. We find, then, in the hands a marked preponderance of flexions under unpleasant stimulation, 78% of the left hand's movements and 69% of those of the right hand being flexions.

Combining the cases of the reactions of the head and hands under the influence of disagreeable stimuli, there were of flexions 53%; of cases of no reaction 29%; and of extensions 26%, or in these 450 cases there was flexion 240 times; no reaction 90 times; and extension 120 times. Leaving out the occasions when no reaction occurred, *there were just 66⅔% of flexions to 33⅓% of extensions under unpleasant effective tone*, a proportion of two to one.

UNDER INDIFFERENT STIMULATION.

In the cases when the stimulated feeling was pronounced by the subject indifferent, that is, neither agreeable nor disagreeable, some interesting results were obtained. Even here the left hand proved readier to react than the right. The left hand under indifferent stimuli showed flexion 34%; no reaction 31%; and extension 34%; or in 32 cases flexion 11 times, no reaction 10 times, and extension 11 times. It will be observed that the flexion and the extension are equal.

The right hand under indifferent stimulation was less mobile. Its results were: flexion 30%; no reaction 50%; extension 20%; or in 20 cases of 'indifferent' stimulation the right hand flexed 6 times; made no reaction 10 times; and extended 4 times. Leaving out the cases when it did not react, it flexed 60% and extended 40%.

The left hand showed only 31% of cases of no reaction under indifferent stimuli while the right hand showed 50%. The head under indifferent stimuli gave 30% of flexions; 30% of 'no reaction'; and 40% of extensions; or 43% flexion to 57% extension, disregarding the immobilities.

The total results for the hands and the head for indifferent stimuli are 31.7% flexion; no reaction 35%; and extension 32.9%; or, disregarding cases of no reaction, flexion 49% and extension 51%. Extensions here then are almost equal, the degree of extension preponderating being too slight to be greatly significant either way. There were in this series 35% of no reaction to 26% under pleasant stimulation and 20% under unpleasant stimulation.

SUMMARY AND ADDITIONAL OBSERVATIONS.

The foregoing results may be seen almost at a glance in the brief table below; it includes only the actual reactions to affective stimulation sufficient in size to be recorded by mechanical means; these were in number 764. The cases in which the reaction, if any, could not be recorded numbered 253 in addition, or a total of 1,037 experiments in this portion of the research.

	Stimulation Unpleasant.	Stimulation Indifferent.	Stimulation Pleasant.
Flexion.	66.6%	49%	32.2%
Extension.	33.3%	51%	67.8%
Proportion.	2 to 1.	Nearly equal.	1 to 2 +

These experiments afford therefore a *striking confirmation* of the general theory which we shall use—namely, that there is underlying organic affective movements a strong tendency to extension or expansion during agreeable states, to flexion or contraction during disagreeable states, and that in a most literal as well as in the most general and even figurative sense. Other tendencies are shown to obtain, however, some of which conflict with this tendency; such, for example, as the predisposition to move toward an object which attracts attention; the inclination to move away from a disagreeable object; the tendency to make particular movements of adaptation to stimuli, etc. It must be further constantly borne in mind that in all cases the conditions are necessarily inadequate to furnish in the laboratory stimulations which are of an unmixed and strong character suitable to a complete affection of the whole organism or of the whole consciousness. Further circumstances of great interest and importance which cannot be as yet taken into consideration at all are connected with the individual psychology of the persons who served as subjects in the experiments, namely vague but doubtless all-pervading influences due to temperamental or moodal conditions. These offer here, as almost everywhere else in experimental mental science, material for research in a direction of their own, but they can here receive no further discussion, chasms too broad and yawning opening up on every hand.

Let us now look into some of the conditions under which these foregoing results, so strongly and indeed so unexpectedly corroborative of the presumption we are investigating, have been secured. Especially must we consider the relative states of evolution of the different species as regards their emotional habits, and how those of the higher ranges of development have been affected by their present modes of biologic life. For example, the extensor-flexion correlation whose validity we are investigating has been changed in its tenor in the slow course of the innumerable generations in which any species has become what it at this stage is. These changes must be looked at briefly, for they seem to strengthen immensely the force which for biology and reality this dual principle has.

Philosophically speaking, the amœba is the lowest and most simple member of the animal kingdom, because it is exactly one simple cell, of which, metamorphosed in an almost infinite variety of adaptive ways, all animal bodies are composed. In this very animal mass of living protoplasm, then, the pleasantness-extension principle (so to call it, for short) is exemplified in its purest and most typical way, and the whole law is therein revealed. When the amœba's biologic egotism is progressing, when it is in the state of pleasantness or in what to us humans is pleasantness, its fluid bodily mass is extended, whether in feeding, in reproducing, or in ‘enjoying’ the satisfaction which must surely come from its normal activity. On the contrary, when irritated, even by jarring, whenever its self-satisfying course of conduct is disturbed or checked, the animal at once tends to contract into the very form of smallest possible surface-extension, the approximately globular—that is, in unpleasantness it flexes within itself. This is the instance as typical and perfect for the hypothesis as were possible, and the directness and simplicity of its action are remarkable in this at once the philosophic and morphologic type.

As development in any higher species has become more advanced and generally, therefore, more elaborate, complications are encountered which make the empirical phenomena more difficult to see, and which, indeed, somewhat affect their natural purity. There are many reasons for this: for example, one organ must be used for many sets of functions; variations in mood and in temperament change the physical conditions and the affective tastes both at once; structure often makes extension and flexion impossible in this typical perfection; certain series of muscles, etc., grow into those motor habits which their most frequent use has made inevitable, and in the cases in which these habits conflict with a free exercise of this principle, it is the latter apparently which is repressed.

But again, teleologic reasons make the relative perfection of this originally purely protective tendency of greater use in the lower orders of animal life than in the higher. For the greater safety of the unintelligent low forms it is important that unexceptionally the pleasant should be the advantageous, 'expressed' by activity of expansion and of approach, and that the unpleasant should be harmful, correlated in the body by withdrawal and flexion. But with the added intelligence of discriminative power, it might be that the advantageous might well enough be unpleasant and *vice versa*, as we know empirically is the fact is some of the at present highest species. Very often, indeed, with us, unpleasantness is agreeable to us, and even pain may be so far in accord with the nature of the ego at the moment as to advance its purposes and so give the essential satisfaction, if not actual pleasantness, for these two abstractions shade off into each other, and cannot properly be always discriminated.

Thus, the subjects in the above outlined portion of this research could not in every case be certain that the tastes of their organism, so to say, agreed precisely with those of their personal moods at the moment of reaction. These innumerable tendencies, indefinitely opposed to the underlying principle, prevented complete constancy of the reactions, as the representing figures show. Such, to be sure, must continually be found to be the case in every reactive—that is, psychophysical—experiment on the higher organisms. In this case, it will be recalled, the persons whose respective degrees of correlation were recorded were among the highest levels of civilization and development, subjects whom beforehand one might well expect to be as far away as any practically obtainable from forms of life which would show the perfection of this extenso-flexor correlation. The brutes, could they tell what their judgments were in such experiments, or savages, or young children, we may fairly presume would exhibit series of reactions very much more nearly approaching uniformity than even our recorded proportion of more than two to one. On the whole, then, we may grant that the principle has received very strong experimental proof, despite the disadvantageous conditions of a university laboratory research, from the set of experiments just detailed.

The next most mobile limb of the body, although a part exceedingly less mobile and with much more inertia than the fingers, is the arm, and in particular the forearm. This, therefore, was next studied in respect of its sensitivity and extramotion to agreeable and disagreeable affections of the general consciousness. In this set of experiments (conducted, like all the rest save the series just described, by the author alone) the investigation was directed toward learning how far the motor principle in question varied extensive movements *voluntarily* made by the subject, the immediately preceding set having dealt with reactions quite involuntary and most often quite unknown even to the consciousness of the subject. For the carrying out of this purpose apparatus was arranged in a manner which will immediately be described. It has the merit of simplicity and the requisite of precision. The apparatus consisted of a sheet of heavy white pasteboard fixed horizontally on the top of a rather high table. On this smooth surface radii five degrees apart and covering a total circular arc of 110° were drawn in heavy black lines, each marked in degrees counting from both ends of the arc. At the centre of this segment a large cylinder of cork was placed and so fixed on a pivot as to rotate evenly and easily, the top being hollowed so as comfortably to fit the olecranon process of the ulnar bone. To the palm of the left hand of the subject, seated comfortably in the position proper for easy movement of the forearm over the table, a sharp-pointed brass stylus, fastened to strong but light splints on either side of the wrist, was fixed firmly with bandages. These narrow splints served to keep the hand rigid and straight in relation with the forearm (without motion at the wrist), and the stylus in a constant position on the palm of the hand. Thus when, as was always required of the subject, the stylus was vertical, its point, gliding just over the surface of the card, was a true indicator of the movements of the forearm about its fulcrum, the elbow-joint. A bit of wood held in the hand of the operator served to limit the varying backward and forward movements of the arm when the norm for imitation was being taken. Between the subject and the table was placed a screen made of black cardboard, bellied out before the face, and with an aperture in the centre of this recess 13 cm. square. It was arranged that squares of Bradley's colored papers should be exhibited to the subject through this opening; these were held one at a time by a clip sliding on the smooth upper edge of the screen.

The stimuli employed were these colors and in addition a set of odors. The color-stimuli were fifty-five in number, and consisted of Milton Bradley's standard saturated colors, intermediate colors, and one tint and one shade of each color. Each of these stimuli-sheets had a consecutive number on its back by which it could be recorded in connection with the reaction caused and the judgment as to the affective tone produced. Besides these colors there were employed as stimuli, but not quite as much, a set of twenty-five odors, the same series already described above with the addition of two more pleasant numbers, jockey-club and lily-of-the-valley perfumes. These were contained and applied as already described.

Such being the simple-enough apparatus of the present set of experiments, the methodical routine of conducting them was briefly as follows: The subject being seated so that his or her elbow rested in the padded and pivoted cup and in such a position that extreme extension of the forearm placed the vertical stylus on the radius marked 0°, the stylus point was placed by the experimenter carefully on radius 1, 2, 3, or 4, each representing as many decimals of circular degrees; this was the starting-point of the performance of the norm. Then the subject, the eyes being closed, carefully attending to the kinæsthetic experiences in his arm, flexes the forearm until the stylus touches the wooden ruler held by the operator about 40° further round. The hand was then quickly carried back by the experimenter to the starting-point (this being always a purely passive movement on the subject's part), and, a sheet of the color-stimulus having been meanwhile placed in the aperture of the screen, the subject opened his eyes upon the stimulus and at once imitated the best he (or she) could the previous flexion at one sweep and at a constant rate of movement. The angular difference, plus or minus, between these two voluntary movements (the norm being always the former of the two) was then recorded in circular degrees, together with the subject's judgment as to the pleasantness, indifference, or unpleasantness of the stimulation, the number or identity of the color, and the portion of the whole arc covered by the movement as to its position. The judgments were made in seven grades, each having a symbol, the digits from one to seven, the empirical maximum of agreeableness being called one, indifference four, and the maximum of disagreeableness seven. This notation is both convenient and precise. The procedure when odors were employed as stimuli was the same, save that they were applied to their appropriate end-organ. Here, too, as always, the eyes were closed, that the attention might be the more fully on the joint and muscle sensations in the arm.

This same process was then effected in the direction of the arm's extension, the stylus being placed at 8, 9, or 10, the voluntary movements then ceasing about 40° further on toward complete extension of the forearm. Thus, continually, as long as the subject's sitting lasted, a flexor and an extensor imitative affective movement were alternated, a norm in each case, of course, preceding each imitation, starting at various degrees both of flexion and of extension.

Besides the reactions made under stimulation, many experiments were performed which should serve as a necessary foil or basis for judgment of the affective reactions. These were conducted in precisely the same manner in all respects as the others, save for the absence of stimuli; in these the imitation was of course more perfect. The results are tabulated below under the head of 'normal experiments.'

The left arm only was employed in this series of experiments, it having been proved in the last set to be more biologically naïve, less trained and 'civilized,' than its homologue the right. Throughout, great care was exercised to stop the work at once when either the arm or the olfactory or visual end-organ was in any appreciable degree fatigued.

The subjects in this experimental set were eight in number, all students in the Laboratory, and two of them (women) students of Radcliffe College. In age the subjects ranged between twenty-one and forty-five years, two having the latter age.

The subjective reports of introspection as to the methods by which the proper length of the second or imitating movement was determined have a bearing here and are in part briefly summarized. One subject reported that his attention was upon the movement directly more when the stimulus was unpleasant than when pleasant; he was guided in his movements by muscle and joint sensations, visualizing only when a position was suggested to him. Another subject said he noticed that a degree of inhibition was required to stop the arm at the proper place when the stimulus was unpleasant, there being then also more of a tendency to jerkiness; he visualized the spot where he judged the movement should end. Another was guided by 'muscular sensations expressed in visualized distance.' Quantity of muscular and joint sensations, then, and a visualized image of the hand at the final position, were the two more ordinary criteria for making the judgments of distance.

The total number of movement-experiments made in the series was 1,328, divided as follows: With colors as stimuli, 574; with odors, 326; and without stimulus, 428.

It is noticed that the conditions of these experiments were made very different from those of the preceding set on the hands and head. Primarily, these were *voluntary* movements of a limb of considerable size made through a rather extensive arc, disturbing elements arising from the arm's momentum being therefore unavoidable. But it was especially desirable that the influence of feeling-tones, pleasant and unpleasant, on just such voluntary actions as these might be investigated, emotional extramotion being constantly to a greater or a less degree superimposed upon our ordinary habitual voluntary movements. The complexities of voluntary acts so greatly exceed those of reflex or of subconscious movements, that one would in advance not expect any such uniformity of reaction to affective organic tones as was exhibited by the mobile fingers and head when left free to follow the underlying tendency of their motor nature. There are present in this set of reactions, then, conflicting conditions which make the probability of uniformity of reaction more improbable even than in the last experimental set, while all the inevitable deficiencies in the artificial imitation of emotional tones are present to disturb here, as there.

The actual results obtained from these 1,328 reactions are summarized, as far as figures allow, in the accompanying table. The numbers indicate percentages; very nearly the same number of judgments of pleasantness as of unpleasantness were recorded, the odors and colors judged as 'indifferent' being, on the other hand, considerably fewer.

Voluntary Arm Movements.

		EXTENSION.		FLEXION.	
STIMULUS.	MOVEMENTS.	EMOTIONAL.	NORMAL.	EMOTIONAL.	NORMAL.
Pleasant	Less.	33.3		48.6	
	Equal.	2.8		4.1	
	Greater.	63.6		47.3	
Indifferent	Less.	29.0	30.0	43.1	40.9
	Equal.	9.1	3.3	11.1	4.3
	Greater.	61.9	66.6	45.8	54.5
Unpleasant	Less.	33.9		47.4	
	Equal.	2.8		4.2	
	Greater.	63.3		48.4	

It will be observed from this table that there exists a very large tendency to exaggeration of the imitation in case of extensor movements of the forearm, these being larger than the norm in these experiments in fully two cases out of three. The flexor movements show nothing of this, but are about half too large and half too small, as the theoretical probabilities would prophesy. This extensor excess is doubtless due to the inherent over-pleasantness of extension; the greater power of the flexors precludes any explanation on the ground of sthenic agreeableness. Furthermore, it is worthy of note that this extensor excess was less in the emotional reactions than in the normal imitations where division of the attention between the arm's sensations and a stimulus was absent.

Disregarding the normal, test, movements and comparing solely the results as to the effects of pleasantness and of unpleasantness on the length of the voluntary movements, the following numbers are collated, forming a percentage

SUMMARY.

MOVEMENTS.	STIMULUS.	EXTENSION.	FLEXION.
Greater	Pleasant.	63.6	47·3
	Unpleasant.	63.3	48.4
Less	Pleasant.	33·3	48.6
	Unpleasant.	33·9	47·4

There is, then, in every one of the four cases compared a numerical difference which is invariably in favor of our principle. As regards the extensor movements this is exceedingly small, and even in theflexor reactions little significant, when compared with the results obtained from the experiments with involuntary reactions of the fingers. In words this summary means this: In simple voluntary imitative movements of the forearm, the extensor movements are more often greater than less under pleasant stimulation or feeling, and more often less under unpleasant feeling; on the other hand, flexor reactions of the forearm are more often greater under unpleasant feeling, and less under pleasant feeling. More simply stated yet, agreeableness makes extension of the forearm proportionally greater and flexion proportionally less, and *vice versa*. Under the circumstances of a large limb moving with no little momentum through a considerable arc this completeness of correspondence with our principle in each of the four cases is both interesting and conclusive, however small the preponderance which the figures prove. The tendency as a basal organic law is again by this set of experiments certified.

Logically to complete in a systematic way the study of these affective influences, a somewhat similar set of experiments was next undertaken to investigate how the *leg* reacts to pleasantness and to unpleasantness, and again involuntary movements or reactions were to be studied. Whenever the will is active in determining effective extramotions, its effect it seems is chiefly of an inhibitory sort (a fact which is important for judging the foregoing results). On the other hand, the leg is adapted by its very perfect hinge joint at the knee to express involuntary motor tendencies, although its considerable weight and consequent inertia would seem to make probable a record of only the most powerful of the motor impulses.

In the last third of this portion of this research then, the intent was to register any involuntary movements of extension and flexion of the leg in relation to the thigh which might occur. The left leg was employed, for reasons like those which proved the left arm more suitable for the purposes of studying instinctive actions,—namely, its less degree of 'civilization.' Here we are studying some of the largest muscles of the body and quite the largest of the joints. The apparatus employed to adapt these conditions to those of the possible reactions may be described thus:

The subject sat in a chair set on a table, his left thigh comfortably supported in a canvas swing hung between wooden supports, the leg thus being able to swing with the utmost freedom to and fro. To the lacing of the shoe (by this means its pressure on the foot was equalized) a cord was attached which passed over a firmly-supported pulley and was tied to a balancing weight, this pulley being centered so as to be equidistant from the extremes of the foot's motion, and the weight being adjusted for every different leg and foot so as just to balance it and allow of extensor movement being as easy as flexor. To the tibial crest on the anterior surface of the leg and just beneath the patella tendon there was bound by tight but comfortable bandages a short pendulum swinging on fine knife edges and attached to a scale graduated to degrees from either side of its central point; the scale then was rigid with the swinging leg, while the pendulum served as a plummet quickly stopping at the vertical position. This pendulum and scale was made by W. Cehmke in Berlin, and with its many joints and adjustments is a useful instrument. The scale, swinging with every movement of the leg, indicated at a glance, to a small fraction of a circular degree, the exact position in which the leg stopped swinging in relation to the place at which it was before the stimulation was made.

The stimuli employed in these experiments also were odors, they being the same used before with the addition of an extremely agreeable one, attar of rose; this made their number twenty-six, ranging from the most generally liked to some of the most disagreeable of smells—aside from disgusts, whose use had no purpose in this research.

The subjects this time were ten in number, properly all men, mostly those employed on former occasions, young men of the Laboratory.

The routine of the experimental method was as follows, the subject having been carefully instructed to sit in the chair in a constant position and posture, and to attend solely to the odor during each experiment, with eyes closed, and without voluntary movement of any muscle. The pendulum-scale being bound on and the leg and foot very carefully balanced by the required weight at the end of the cord, so as to swing one way no more easily than the opposite from a certain point midway between the extremes of flexion and of extension, the scale was adjusted so that swinging normally the leg returned to the exact zero-point each time. The subject then took from the rack of vials, convenient to his right hand, one of the odors, while the experimenter drew down the weight balancing the foot and leg to a support beneath, by this means making the leg's initial elevation and consequent swing on release of the weight constant in amount. At this, the signal agreed upon, the subject applied to his nose the stimulus as the leg was let swing by the experimenter, the stimulation being continued during the two or three seconds or more required for the foot to come to complete rest. Then the degree of the limb's extension or flexion was recorded in degrees, together with the subject's judgment on the tone of the stimulus, whether pleasant or unpleasant, in one of seven degrees as before, and the number of the odor. Great care was used to at once stop the experiments whenever either nose or leg gave any feelings of fatigue; this often occurred in twenty minutes or a half-hour, thus making these, like all experiments with odors, comparatively slow work. The various stimuli were used in nearly equal extent, except that the more powerful ones being properly those of greatest value, they were employed more often than the commonly indifferent members of the list.

Normal experiments, that is, reactions when no stimulus is given, were not required in this set of experiments, for the reason that the scale was each time adjusted to the zero-point, to which the normally swinging leg would in every instance return. The number of the experiments was 801. Their numerical results are given in the table which follows:

INVOLUNTARY LEG MOVEMENTS.

STIMULUS.	PLEASANT.			INDIFFERENT.			UNPLEASANT.		
Reaction.	Extnsn.	No mvt.	Flxn.	Extnsn.	No mvt.	Flxn.	Extnsn.	No mvt.	Flxn.
Cases.	138	111	139	14	17	24	65	113	180
Per cents.	35.6	28.6	35.8	25.5	30.9	43.6	18.2	31.6	50.2

Expressing these numbers in words, pleasantness causes the leg to extend in 35.6% of the stimulations of that tone, to flex in 35.8%, and neither in 28.6%. Unpleasantness caused flexion in 50.2%, extension in only 18.2%, and no change of position in 31.6%. Odors which were called indifferent in affective tone caused extension in 25.5% of their cases, flexion in 43.6%, and no reaction either way in 30.9%. It is these last movements, those under indifferent stimulus, which are properly to be taken as the norm of the experiments, the affective state being in these cases *as in an emotion without appreciable algedonic tone*. We should not properly compare the reaction obtained with that made when no stimulus at all was applied, but rather with the reaction from an indifferent stimulation, the conditions of the method being such as to allow of no reaction of any sort when no stimulus was applied. An indifferent stimulation caused a neutral affective tone, and this alone is the normal standard of the reactions.

Comparing with this normal standard the results obtained under pleasantness, we find that there then were more extensions than flexions: in the ratio of 35.6 to 25.5. On the other hand, unpleasantness caused more flexions than extensions (as 50.2 to 43.6) and also fewer extensions (in the ratio of 18.2 to 25.5). Again, disagreeableness gave rise to flexions in over half the reactions under it and only 18.2% of extensions. These comparisons are observable at a glance almost in the following scheme or

Summary.

Reaction.	Pleasant.	Indifferent.	Unpleasant.
Extension.	35.6%	25.5%	18.2%
No movement.	28.6	30.9	31.6
Flexion.	35.8	43.6	50.2

Here again we see further demonstration of the correlation between tone and bodily movement, the flexor tendency in this case being emphasized. From these experiments also were compiled data which would be valuable in a study in individual psychology or of temperament, for the ten subjects varied greatly in their modes of reaction and in the numerical results from compilation of their leg movements. One subject reacted to pleasantness with 53.3% of extensions and 31.6% of flexions, while under unpleasantness his leg exhibited 38.8% of flexions to 22.4% of extensions. This was the subject all modes of whose reactions most favored the principle. Again, one or two were relatively deficient in extensor reactions under all circumstances.

It would appear from the collected figures that the individuals who react least, who are least emotional, when they do react are strongly of the flexor tendency. This would seem, for several such reasons as are above suggested, to be one of the most fertile fields for the study of individual psychology of the affective aspect of mind.

The concluding portion of the experimental part of this present research was conducted in the Psychological Laboratory during the first three months of 1899. It relates to the vascular and respiratory movements and conditions concomitant to joyful consciousness. These changes, as Lange has shown, probably take a preponderating part in the experience of emotion, making up much of its substance or mass, and it is unfortunate that our access to these variations should be at present so imperfect and so uncertain, in a way, in result. On man, who can express in words his feelings to us, we cannot adequately experiment; while the lower animals, which, unfortunately, some persons deem it proper sufficiently to explore and study, cannot adequately convey to us the tone of what they might feel, whether pleasant or unpleasant, weak or strong. On this account affective psychology seems greatly handicapped until some possible new mode of approach to the vascular secrets of higher organisms shall be evolved. As it is, we study really only some indefinite exponent, so to say, of the real variations, and for this relation no formula exists. A sphygmogram represents the emotional changes in the vascular organism in some degree quantitatively, but none too well; indeed, no single curve, especially in one plane, could ever represent the mechanical conditions of a hydraulic system so complex.

Eleven subjects were employed in this research, all, save one, students of the Columbia courses in Psychology, all males, and with ages between twenty and thirty years. There was among them a considerable variation in emotivity.

To study the vascular movements in joy a sphymograph was employed, this instrument and method being better indicative of the blood changes as a whole than the plethysmograph. The sphymograph employed in part was of the Marey pneumatic form, the greater part of the work, however, being done with a mere simple brass tambour with a light cork rod attached by a wide base to the rubber's surface; this tambour was supplied with an arm by which it might be tightly clamped and so applied by means of rods and universal joints to any artery on the surface of the body. This device was found to be of far greater utility for work like this than the instruments made especially for each artery, but which are difficult and slow of adjustment and often uncomfortable to the patient and constrictive of the artery investigated.

The instrument with which the studies on the respiration were made was the excellent and simple pneumograph of Fitz, consisting of a thin rubber tube about 3 cm. in diameter and 20 cm. long, stretched over a light brass-wire spring, this being closed at one end and connecting with the transmitting tube at the other; straps fastened this pneumograph around the abdomen of the subject at any place and at any degree of tension.

The sphygmograms and pneumograms were traced on gas-smoked paper attached by springs to the surface of a kymograph drum. This drum was 95 cm. in circumference and 26 cm. in width, and was rotated on a vertical axis by an electric motor (with gearing to reduce the speed), once in about four and three-quarters minutes, or about one centimeter in three seconds, the record being produced so as to read naturally, that is, from left to right. The recording-pens were actuated by Marey tambours attached to a carriage controlled by the groove of a screw in such a manner as to descend a proper amount at each rotation of the drum, the amount being adjustable. By this means a continuous record of a half-hour or more was possible. The pens were of very thin sheet copper, giving a delicate and finely detailed curve; a time-line, run by a pendulum beating seconds in another room, served also as a base-line from which to measure the departure of the curves.

The stimuli employed in this part of the research were, in the main, those used in the preceding portions, and the same difficulties are present. The constant endeavor was to induce in the subject pleasant feelings best described as joyous by whatever means this could be brought about. Induced states of humor and of pleasantry were frequently occasioned by reading to the subject humorous or witty passages from books, the telling or reading of jokes, and by other similar means. In other cases the subject was directed suddenly to recall to mind some well-remembered state having a strong pleasant or unpleasant tone, some pain or pleasure, some agreeable or disagreeable tone still fresh and strong in the mind, aiding the effect by autosuggestion that the conditions were then immediately present. This was found, as in a former set of experiments, to be a ready and powerful stimulus in most cases. Oftentimes these purely mental or imaginary stimuli were assisted by real odors, such as were used before, among them being attar of rose, heliotrope, ammonium valerianate, and tincture of asafœtida. More pleasant stimuli than unpleasant were employed in the present experiments, descriptive facts as regards joyful intramotion being here what were desired, with a knowledge of the nature of laughter. The same (hypothetical) gifts of money above considered were employed likewise in some cases with interesting results. A condition of mirthful and exhilarated good humor or delight, as the nearest laboratory approach to natural and instinctive joy, and induced and ended as suddenly as possible, was the state which was repeatedly produced and studied in the various subjects.

The arteries from which the results are combined were the radial, the carotid, the temporal, and the tibial, which in the main showed no marked difference in the type of their reaction to the psychic tones. Certain dissimilarities between the central and peripheral arteries seem to obtain, those nearer the brain showing the most marked reactions.

The problem here, then, was to learn the vascular and the respiratory extramotions of joy. Each of these, so far as represented by the record-curves made from them, may be studied in three ways or directions with more or less success: the pulse as regards its general pressure, impact upon the arterial walls, and frequency; the respiration, in respect of its time-occurrence, the relations of inspiration and expiration, and the quality of each stroke. These results of these experiments may be given *quantitatively* as well as qualitatively in a space proportionally small. It is by such comparative and quantitative conclusions that knowledge about the emotions will gradually increase.

The most universally constant effect of pleasant simulation upon the *vascular system* seems to be an increase in arterial pressure throughout the body and especially upon its surface. This is represented in the sphygmogram by a rise of the pulse line in the shape of an arch varying in altitude from a portion of a millimeter to a considerable fraction of the centimeter with the recording instruments here employed. The condition is the accompaniment as well of all active mentation whether affective or cognitional, but in no case else seems so emphatic as in suddenly induced conditions of a joyful nature, being then constant and often marked. The carotid artery seems to display this reaction to the best advantage.

A reaction due doubtless more directly to the increased cardiac kinetic energy is the extension in the width of movement which the pulse pen makes at every beat, this being found also in disagreeable states in about the same degree. Owing probably to mechanical conditions, the widening of the excursion is not seen to be a constant phenomenon, but is often marked. It would seem that the increase in question as measured on the sphygmogram may be stated fairly at twelve per cent. over that of the non-affective normal excursion, or when the mind is not forcedly active in any way, ranging between ten and twenty per cent. This increase is more or less dependent on the quickness with which the heart's left ventricle contracts, and this varies with the tension of consciousness, joyful tension being particularly easy and emphatic.

Further evidence of the sthenic nature of emotion when of a pleasant tone is to be found in the acceleration of the pulse occasioned by the stimuli employed. It is probable that in this reaction is also to be seen evidence of the excitement factor of beneficial emotion, this being sometimes the only appreciable quality of an affective state when the pulse-rate is distinctly increased. This effect we may report quantitatively, the instances cited being average examples of the total number of such reactions. The counting was done in many cases, and an equal number of seconds of normal pulse and the joyful pulse were compared. In only two or three cases was no acceleration perceptible; it ranged from three per cent. up to twenty per cent., with an average acceleration of about seven per cent. The subjects varied little in this respect. It was observable that pleasant odors did not occasion the increase that imaginary and immediate psychical tones produced. As was the case also in regard to the width of the pulse curve, the acceleration was most marked at the beginning of the stimulation, sometimes then reaching for a few beats an acceleration of forty or fifty per cent., thence continuing its emotional rate until it decreased rather suddenly in general when the affective state was forcibly closed by change of attention-point.

The *respiratory* extramotion of joy seems to be somewhat less definite and simple than that of the pulse. This is as would be expected *a priori*, for while the heart is probably controlled chiefly by one nerve arising from one spot in the medulla, the respiration is directed through the action of several nerves having among themselves much more various relations with the cortex, notwithstanding that the respiratory center proper is localized somewhat restrictedly in the calamus scriptorius; the ordinary muscles of the process certainly have far wider and more various control.

On the application of a pleasant stimulus such as has been described, the first inspiration is usually much deepened over that of the normal, indicating an enlargement or extension of the thorax. This increase as measurable on the pneumogram record varies from perhaps ten per cent. to ninety, with an average fairly statable as about twenty-five per cent. It is to be noted here as in the case of the pulse that the effect of pleasant odors, giving what may at times be properly termed pleasure, sometimes of a marked degree, do not produce this inspirational reaction with the regularity that ideal stimuli and joyful occasions proper produce, and this in spite of the fact that the natural tendency is to inhale a delightful odor as deeply as possible. The natural rhythm between the respiration and the pulse tends to be retained at all normal times, and to this must be ascribed the observed fact that the succeeding respirations, after the first, are smaller, then requiring less time, and so being able to keep up with the accelerated pulse.

It is observable in addition that in delight the respiration tends to be carried on deeper in the chest than during unpleasant states, indicating a more or less continued contraction of the diaphragm. This unusual exercise of the chief muscle of respiration is in itself a pleasantness until prolonged to fatigue, as may be experienced by any one at will.

Whether or not acceleration of the breathing is to be considered a concomitance of joyful feeling it is difficult to say from study of the record curves of this research. At times an increase in the rate is distinctly apparent, but at others the deepening of the breaths seems to cause an opposite effect. This seems to be borne out by observation at large, an excited joy producing sometimes a rapid respiration-rate, especially when the emotion is of a light degree, while deep joys, containing little of the excitement element but much to affect profoundly the welfare of the self, exhibit deep, slow breaths. The pleasant odors employed as stimuli seem to occasion a slowing rather than an acceleration, while the purely mental modes of stimulation seem in a long run of seconds to accelerate the breathing into rhythm with the hastened pulse.

Laughter was described to some extent by the tracings on the smoked paper. First there is a very deep inspiration, which is usually in part only released before a series of irregular, very short vibrations ~~of the pen~~ occur, ~~indicating a clonic spasm of the diaphragm~~; at the end of three or four of these smaller contractions a somewhat larger one occurs, followed by a forced expiration; then several more spasmodic movements occur, ended, in a mild laugh, with a strong expiration and a deep compensatory inspiration when return to the normal amplitude takes place, usually with a feeling of decided pleasantness from relief from the unwonted fatigue of the diaphragm and the smaller accessory muscles of respiration. During an attack of laughter nearly every muscle of the body is in some degree innervated, the extensors in particular, as has been shown, and the pulse-curve therefore is inevitably disturbed beyond the possibility of translation. The diaphragmatic vibrations chiefly productive of a moderate period of laughter seem to be in number from ten to thirty complete vibrations, with an average of about eighteen. The amplitude of these clonic contractions of the diaphragm is ordinarily not more than a tenth or a fifteenth of the normal contraction, but, because of their unwonted tension, they are quickly fatiguing and therefore soon cause, if continued, a 'doubling-up' of the trunk to relieve the strain on the diaphragm as well as upon the other extensor muscles of the body.

We have now obtained from thousands of experiments on actual subjects under careful scientific conditions evidence from practically every accessible portion of the body that the pleasantness-extension theory has its basis in the very nerves and tissues of the organism. This evidence has in fact been obtained under adverse conditions, the subjects being highly cultured civilized adults, and the necessary laboratory limitations making the stimuli employed very far from those acting in natural conditions, all the states studied, moreover, only approximating those of 'real life.' We will now attempt to apply these basal principles, and to point out, in some measure at least, why the familiar general manifestations of joy are what they are known, instinctively almost, to be.

The extramotion or 'expression' of joy is characteristic in about an average degree, there being emotions (as anger and fear) which have more characteristic expressions and others (such as love and hate) which have less. The bodily correlation which is most characteristic of the emotion in all its degrees is the smile and laugh, while the rest of joy's extramotion is composed of actions performed also to some extent in other emotions, and especially in those in which pleasantness is prominent, as pride and love. Preyer in his 'Die Seele des Kindes' reports that the mouth of his child smiled during sleep (from the reflex pleasantness arising from a hearty meal) on the tenth day after birth. Joy from music, from being spoken to, from a bright face, caused lively movements, the general accompaniments of pleasantness, in the first week. Even on the first day Preyer thinks pleasantness caused wide opening of the eyes, and a little later an animated gleam in them (from activity of the lachrymal gland). Alternate stretchings and bendings of the extremities in pleasant bath or when the piano was played were regular activities from the second month, 'multiplied and associated with very loud joyous shoutings' later on, as in the seventh month, with lively kicking when put to bed, when fed, and when placed in a strong light, the last being a phenomenon observed in adults by the present writer on several occasions. Only in the ninth or tenth month do there occur striking of the hands together and laughter for joy, quite different from the reflex laugh of earlier months. Monkeys and dogs are often seen to smile, while birds come very close to it sometimes, as well as, and probably more properly, other animals.

Laughter is produced by clonic spasms of the diaphragm and other respiratory muscles, of most of the facial muscles, especially those round the mouth, while there often obtains an increase of intra-orbital pressure which causes a slight protrusion of the eye and finally an excretion of tears. The upper part of the mouth and the lower jaw are drawn upward and the head often backward, the back too being straightened even to the commencement of flexion backwards, until (and this often happens soon) fatigue-pain in the diaphragm and accessory abdominal muscles causes a marked ordinary flexion of the trunk for its relief, as above observed. The vascular pressure seems meanwhile to be usually increased with consequent general glandular and visceral activity.

The precise muscles involved in laughter as the typical joyful extramotion may be set down as follows: their location and even their names have interest in the light of the principle implicated in their action here. Besides these listed there are many others, of course, especially in the neck and about the vertebral column, which are not mentioned, they being too numerous and too small.

The Diaphragm and the nine or ten other muscles productive of respiration.

Risorius, Zygomaticus major and minor, Levator labii superioris, Levator labii superioris et alæque nasi, Levator menti, Orbicularis palpebrarum, Pyramidalis, Dilator naris posterior and anterior, Superior rectus oculi, and Corrugator supercilii; these in the main make the facial extramotion.

Splenius capitis et colli, Obliquus capitis superior, Rectus capitis posticus minor, Erector spinæ, Biventer cervicis, Complexus, Trapezius, and Cervicalis ascendens; these extend the head.

Longissimus dorsi, Multifidus spinæ, Sacro-lumbalis, Accessorius ad sacro-lumbalis, and Spinalis dorsi; these, with various large muscles joining pelvis and thigh, extend the trunk.

In joy (as in extreme grief and suffering sometimes) cries of a very high pitch are uttered, the heightening being due probably to contraction of the Crico-thyroideus and Crico-arytenoideus posterior muscles.

A general synopsis of the extramotion of joyous emotion includes movements of bodily parts such as these: When very violent, very general bodily activity of undefinable variety is the rule in those animals and conditions where it is pure joy. Dancing, jumping, shouting, running, clapping the hands, turning somersaults, less definite universal movements occur, in short any violent exercises conducive to the excited and sthenic feelings of the inner experience, all interacting with the heightened vascular and respiratory and glandular functions. Sir Humphry Davy, it is often related, danced with joy in his laboratory when he discovered sodium, while all the world knows how Archimedes on a certain historical occasion ran wildly through the streets shouting Eureka!

In the lesser and ordinary degrees of joy and often, for reasons we have already rehearsed, when the inner joy, so to say, is really excessive, all this is modified and mollified to a set of phenomena appearing quite different. There are smiles or the more complicated laughter, accompanied by all the numerous signs of psychophysical excitement which every emotion implies. The eyes are brighter and the upper eyelid elevated, as also are the brows, the skin over the glabella, the upper lip, and the corners of the mouth; while the skin at the outer canthi of the eyes is puckered. The nostrils are moderately dilated, the tongue slightly extended and the cheeks somewhat expanded, while in persons with largely developed pinnal muscles, the ears tend somewhat to incline forwards. The whole arterial system is dilated, with consequent blushing from this effect on the dermal capillaries of the face, neck, scalp and hands, and sometimes more extensively even, from the same cause, the eyes slightly bulge. The whole glandular system likewise is stimulated, causing the secretions—gastric, salivary, lachrymal, sudoral, mammary, genital, etc.—to be increased, with the resulting rise of temperature and increase in the katabolism generally. Volubility is almost regularly increased, and is indeed one of the most sensitive and constant of the correlations in emotional delight.

Aside from these extramotions which are general to joy, there are for every special-instance of it certain sets of actions which correspond to the objective situation which is the affective state's occasion, all, however, having the common quality that they are directed in such a way as to bring the subject into closer and more intimate relations with this object, but either literally or figuratively as the case may be.

Now, all these varied and ever-varying movements, so great in number and so unique in themselves as to be well-nigh indescribable as they appear in life, seem to have one common characteristic in so far as they are undisturbed by conflicting principles of action or restraint or by disturbing tones of feeling. They are all at once proofs and products of the biologic principle that pleasantness is correlated in living organisms by vascular, muscular and glandular extension or expansion, both literal and figurative. Joy, in other words, is an extension or expansion of the personality in both its aspects, mental and bodily. The mental expansion is manifest in the fast talking, singing, shouting, whistling—by one's 'good spirits' which stand for an enlarged viewing for the time of life's broad good. There are *feelings* of well-being, of self-confidence, of freedom, and of power. There is *willing* of kind, great, or famous deeds, of conduct which shall make the delightful experience permanent to the enlarged benefit at least of the agent. There is *cognition* of practical optimism, while it lasts, that it is joy itself to be alive. The bodily expansion or extension is manifested by the 'open countenance,' the 'broad smile,' the erect bearing, the expanded chest, by the naturally raised and extended arms and spread fingers, with sometimes separated legs, and extended feet and toes, and in special cases by numerous other bodily extensions, such as, in brutes, the erect tail and proboscis. Again, the frequent tendency to rapid walking and running or riding, rowing, sailing, the piano-playing, waving of flags, throwing upward of hats, jumping, shouting,—all these and other like movements display activities as far as possible from the life of grief.

But in a sense much more particular than this is the extramotion of the emotion of joy the result in the main of extensor movements, for, properly classed, that is with due reference to morphological principles, practically all the muscles involved are extensor muscles. The muscles which extend the trunk and the head are ordinarily classed as extensors from their effect when contracted. So far as is known to the writer, however, it has never been pointed out that the facial muscles likewise are readily classifiable as flexors and extensors, their mechanical effects not being of sufficiently practical importance in anatomical physiology to make such disposition of them important. It will be shown, however, that the facial muscles are no exception in the data of myology in this regard. A few considerations will, it is presumed, make this statement clear to an extent materially to assist explanation of affective facial expression, thus helping in some measure to reduce the long-standing mystery of the face, the mirror of the soul's affections. These considerations are at once morphological and physiologic.

Even as the affective principles are manifested almost, if not quite, as early as animal life itself, so must one search very early in the embryo for the forms which are determinant of adult structure and function. Function is more primary, properly, than form, and one generation determines to some extent invariably the structural pattern of the next. We must look, then, to the early embryo for the direct correlation of the functions which are to obtain in the adult. Although the primitive streak is at first necessarily straight, folding begins very early, and at the end of the first month in the human fœtus the embryo is bent upon itself to a very marked degree. The curve which forms in the region, afterwards the lower cervical, is for our purpose the most significant, although the growth forward of the anterior cerebral lobes helps in the case of man to make this antero-posterior bending more marked and more physiologically significant. This curvature is in the adult quite patent in all the canals which extend from before backwards in the head. First, in the ventricles of the brain from the third to the 'sixth' down the cord. Second, from the nostrils upward and backward and then downward into the trachea. Third, from the mouth similarly upward and backward and downward into the œsophagus. All of these in a proper philosophic sense outline even in the higher adult forms the curve which at the period of individual development when the functions of the muscular system were being firmly established in the formative plan of the organism is even more plainly apparent. In the forms of a strongly prognathous type of structure these conditions are all emphasized, as they are in animals with long probosces which these muscles in part serve conspicuously to extend. It is at this early formative period that the myological system is beginning to take its basal opposition of flexion and extension, the large muscle of the scalp, the Occipito-frontalis, being the strikingly complete type of this functional division, the frontal half drawing directly backward in a way to tend to extend the face and head.

But on the same dual principle do all the superficial facial muscles act, of course including those which we have above detailed as the facial mechanism of the smile and laugh. The Risorius draws upward and backward the corners of the mouth. The Zygomatici have similar effects upon the jaw. The Levator labii superioris, as its name implies, lifts the upper lip, as does the Levator labii superioris et alæque nasi and in addition the outer and lower walls of the nostrils. The Levator menti lifts the soft parts of the chin. The upper portion of the Orbicularis palpebrarum raises the skin about the eyes, and the Pyramidalis that over the root of the nose. The Dilatores naris lift and expand the lower portions of the nose. The Rectus oculi superior rotates upward the ball of the eye. The Corrugator supercilii raises the inner ends of the eyebrows until the latter are straight. All of these, physiologically speaking, tend to straighten the curve which, morphologically, extends from the upper portion of the vertebral column to the chin, and hence tend to complete the extension of the body.

It is thus seen that the muscles of the face which take part in the pleasant emotions, and particularly in joy, are extensor in the same sense as are those of the hand, neck, back, forearm and leg, although the form of the face and its complex functions make their division in this manner less obvious than the muscles of the rest of the body. They complete and corroborate the condition found experimentally to obtain elsewhere.

The facts are now sufficiently in accord to warrant the maintaining of a postulate important for the 'philosophy of expression,' statable as follows: The pleasantness-extension and unpleasantness-flexion *principle will apply in emotions in general in proportion as they are pleasant or unpleasant to the agent.* Some affections are very pleasant, and these exemplify the principle one way; others are unpleasant, the converse movements and strains are seen; while many which are neither markedly agreeable nor disagreeable will employ this biological tendency to no appreciable degree. This is in the nature of a corollary to the biologic principle which we have experimentally proved. Recent researches by N. Wedensky[1] have shown the complete opposition of certain motor centers in the cortex, excitation of one extensor center augmenting the excitability of the flexor center of the other hemisphere and diminishing the irritability of the flexor center of the same side. This supports our conclusions arrived at empirically and experimentally, and makes more certain still the law which seems deeply to underlie the materialization of the emotions.

The main contentions of this monograph may be briefly restated thus:

Somewhat in proportion to its proper pleasantness, an emotional extramotion consists in expansiveness and outwardly in contraction of extensor muscles; this is true of the smile and laugh of joy, the fundamental emotion of life.

The regular occurrence of habitual inhibitions, due to complex conditions of civilized social development, supplies the apparent deficiency in the kinæsthetic theory of the emotions of man.

[1] "Jrnl. ochranenia narodnago zdravia," 1897. "*Année Psychologique.*" Vol. 4, p. 449.

Analysis discriminates five components of a period of emotion,—namely, psychophysical excitement; various feelings and their concomitant bodily movements and strains; heightened consciousness of the emotion's object as in relation with the subject-agent; often a pleasant or an unpleasant tone of consciousness; and at times increased self-reference.

Any emotion, being biologically in animals, savages, and naïve infants a more or less constant series of phenomena, is theoretically at least susceptible of future scientific determination more or less exact; while the emotional processes of civilized human selves are so complicated by social interactions as to be no longer properly emotions in the biologic sense, but rather concrete expressions of the affective social consciousness at present quite indefinite.

An emotion is an affair invariably of both a mind and a body, practically the whole of the latter of which it regularly implicates in its movements: it is dynamogenic.

Contraction of the extensor muscles is more pleasant in itself than contraction of the flexors, and this fact, together with the general tendency to flexion which a (naturally unpleasant) sudden shock produces, perhaps determined, phylogenetically, the empirical opposed mode of affective extramotion.

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Conduct and the Weather

An Inductive Study of the Mental Effects of Definite Meteorological Conditions.

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PREFATORY NOTE.

This paper, in its present form, comprises little more than half the material actually prepared for publication. At its final compilation the volume was such as plainly to exceed the proper bounds of a work of this character, and a large part was left out, with the intention of publishing the whole, with important additions, in book form at some future time. In the reducing process the introductory chapter was cut in two, those specifically treating each of the classes of data except that of the school children omitted, and the chapter in conclusion much condensed.

I wish here to express my gratitude to the many, in all parts of the country, who have so promptly and willingly lent their aid to the work by answering and discussing the questionnaire; also to Superintendent Aaron Gove, of the East Denver School, the Misses Jones and Adams, former pupils of mine, now teachers in those schools, Superintendent F. H. Brandenburgh, of the Denver Station of the U. S. Weather Bureau, and Chief Howe, of the Denver Detective Service, for material aid in making the studies of the Colorado climates. For help in the prosecution of the study in New York City, I am indebted to Superintendent Jasper, of the Borough of Manhattan Public Schools, the Principals of Public Schools Nos. 10 and 93, the Superintendents of the various Corrective Institutions for the City of New York, officials at the Central Police Station and Department of Public Health, and to Superintendent E. H. Emery, of the U. S. Weather Bureau.

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CONTENTS.

Section I. Introduction, ... 1

Section II. Statement of the Problem, ... 4

Section III. The Meteorological Conditions, ... 17

Section IV. The Public Schools, ... 27

Section V. Summary of other Classes of Data, . . . 60

Section VI. Conclusion, ... 74

Tables, ... 83

Figures, ... 89

SECTION I.

INTRODUCTION.

The modern science of Meteorology, emerging from the mist and darkness of ignorant guess and surmise, has left its path strewn with many a shattered idol. Jupiter Tonans, the Thunderer, Pluvius, the Rain-maker, and a hundred other weather-gods were toppled from their lofty pedestals ages ago, while St. Swithin and his twoscore of saintly colleagues, whose days dominated the weather for the rest of the year, have been quite as surely if more recently dethroned by the delicate instruments and skillful calculations of the modern weather-man.

It is interesting to turn the gaze backward and view in the light of modern scientific research the fallacies that have been corrected and the superstitions that have been lived down. In the centuries that have gone, each event was a portent; Nature's moods were not interpreted in terms of cause and effect. As a consequence, the weather prophets were likely to forecast that which was most wished for, and to lay down as a general, if not infallible law, that which was a mere coincidence of totally independent events.

What the effects of one meteorological condition may be upon another we care little, and we must, at any rate, leave their study to the meteorologist. The effects, however, of those conditions upon members of the animal kingdom, and especially upon man, are of great interest, though not as a means of weather prognostication. In this modern day, when man is being recognized more and more as a creature of his environment, a sequence of personalities, each one of which varies from all the others as the conditions of that environment vary, it has seemed strange that, of external conditions, the weather has received so little scientific attention. Although there is considerable literature bearing directly or indirectly upon the subject, the bulk of it is principally interesting to the physician, treating, as it does, of the effects of weather upon bodily health and disease. Although the health and strength of the body are but other terms for expressing its physical condition, the health and strength of the mind are as directly traceable to the same physical conditions of the body, and it seems probable that mental states, especially emotional states, are affected even more noticeably by weather conditions than are the underlying physiological ones. Of course, the former are affected only through the medium of the latter; but the condition of mind may be considered the dial which indicates on a large scale certain minute changes physiological, and, although in our study the use of terms might imply a direct effect of weather upon mind, an intermediate element, the body, is of course always presupposed.

That there is a relation between weather and mental states, which seems hardly other than cause and effect, has seemed evident to many. A writer[1] in one of the British magazines some years ago very aptly said, "There are many persons who are simply victims of the weather. Atmospheric influences play upon them as the wind plays upon the strings of an æolian harp, with the difference that the latter never utter discords in reply. A leaden sky weighs upon them with a crushing weight, and suggests all manner of unpleasant anticipation. Then the gloomy side of life comes out. The bitter sayings of friends are remembered. The old groundwork of forgotten quarrels is remembered ; uneasy questions arise with regard to the future. One gets tired of life. A sort of indefinite dread is the general mental influence, a faint continuation of the superstitious fancies which mark the childhood of nations and men."

Who has not at times felt this influence? In all the vigor of perfect health it may hardly be recognized; but when the vital forces are depleted by the exhausting effects of a long nervous or physical strain, the influence of this phase of cosmical environment is sure to make itself known. Then come those days when everything is sure to go wrong. How inconsiderate are our friends when the east wind blows and the skies are heavy! If we are teachers, how provoking the pupils sud denly become! How dangerously doubtful seems to-day the venture which yesterday, in the bright sunlight, seemed certain of success!

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[1] See *Once a Week*, Vol. xix.

Definite statements of teachers, prison wardens and superintendents of the insane with regard to these effects will be considered in later chapters of this study; but literature is full of citations which lead us to believe that those who have 'felt most' this great world's pulsations have been perceptibly affected by its temperature. Charles Lamb said, "That nothing less than a sweltering August sky could meet his craving." At such a time, he remarked, he felt himself immortal, "as strong again, as valiant again, as wise again, and a great deal taller.'

The poet Moore rejoiced like a song-bird in the sunlight:

> "No joy like this
> To sit in sunshine calm and sweet.
> It were a world too exquisite
> For man to leave it for the gloom,
> The deep, cold shadow of the tomb."

Byron, too, shows that a bright day had an extraordinary and apparently unusual effect upon him.

"I am always more religious," he said, "on a sunshiny day, as if there were some association between the internal approach to greater light and purity and the kindling of this dark-lantern of our external existence."

He could "bear cold no better than an antelope, and never yet found a sun quite done to his taste."

Shelley loved the intensest ardor of the sun, and wrote many of his best things on the roof of his house near Leghorn, unscreened from its rays. Rousseau, like him, used to bare his head to the sun. As soon as the days began to turn, the summer for him was at an end: his imagination at once brought winter.

Southey, during one of his temporary visits, in England, after a long sojourn in Italy, very forcibly expresses himself in a letter to a friend: "I have lived too long abroad to be contented in England. I miss the sun in heaven, having been upon a short allowance of sunbeams for the last ten days, and if the nervous fluid be the galvanic fluid, and the galvanic fluid the electric fluid, and the electric fluid condensed light, zounds! what an effect must these vile, dark, rainy clouds have upon a poor nervous fellow like me, whose brain has been in a state of high illumination for the last fifteen months."

Shakespeare himself was keenly observant of the effects of weather. One of the most striking examples of this is in 'Romeo and Juliet.' The fatal brawl in which Tybalt is slain is precipitated by the effect of the temperature upon the principal actors. Benvolio realized that possibility, and, in an attempt to restrain his lively companion, said,

> "I pray thee, good Mercutio, let's retire;
> The day is hot, the Capulets abroad,
> And, if we meet, we shall not 'scape a brawl,
> For now, these hot days, is the mad blood stirring."

But his warning was disregarded. As a result, when the Montagues and the Capulets met, they fought: Mercutio lost his life; Romeo was banished for slaying Tybalt; Juliet was forced to take the potion to avoid a hateful marriage with Paris during her lover's absence, and was discovered, apparently dead, by Romeo, who killed himself; while Juliet, by awakening, completed the tragedy, 'falling dead on the body of her lover:' terrible results of this one hot day.

In this case, as in many others, subsequent investigation has tended to corroborate the wonderful power of observation of this great master. In the records of the public schools we may feel the pulse of the pupils, in those of the penitentiary we may observe the fluctuations in the temper of the inmates, and in the journals of the police courts find relations, too obvious to be regarded as mere accidents, between the 'mad blood,' and the temperature and other weather conditions when most it was 'stirring.'

SECTION II.

STATEMENT OF THE PROBLEM.

In the following chapters I have attempted, in an inductive manner, to throw some light upon the peculiar effects of weather conditions on mental states. That there are such effects no one can doubt. Dr. Johnson may treat them with ridicule, and the strong and robust be scarcely sensible of them, but the great majority of us are fully alive to their power. Yet modern science is not satisfied with a mere knowledge of their existence. Generalizations will no longer suffice in answer to its scrutinizing inquiry. Is the cause capable of analyzation into components, each of which may contribute in its peculiar way to the indicated result? To this our answer is, yes. Those states and conditions, mutations and changes in our cosmical environment to which we give the name weather, do not form a unit, but a composite. The various meteorological conditions, ringing in as they do combinations innumerable, are the ever-changing elements of the cause whose relation to human conduct and emotions we are attempting more definitely to define. It is, for the most part, a study of those weather components and their discernible relations to human activities which this paper treats. Yet it may be necessary at the outset to define our use of the term 'cause,' as applied to the weather or its meteorological components. We do not mean a primary, but a secondary factor. In none of its phases, probably, is the weather the inciting cause to action; but under some of its conditions the mind seems better able to withstand an impulse than under others. In this sense only do we mean cause,—that in the varying conditions some are presented in which impulse to action seems more liable than usual to overcome an ordinarily overpowering inhibitory force. Such a condition might be said to be the cause of the action.

The problem carried on is twofold: first, the tabulation and discussion of a questionnaire sent to nearly two hundred teachers, of all grades, from the kindergarten to the high school, superintendents of asylums and reformatories, and wardens of prisons and penitentiaries; second, an inductive study of several hundred thousand data of classes mentioned below, comparing the occurrence of data of the various classes studied, under definite meteorological conditions, with the normal prevalence of those conditions.

The letter sent out with the questionnaire, and the questionnaire itself, are as follows:

COLUMBIA UNIVERSITY, Dec. 15, 1898.

Dear Sir: I am making a study of the possible relations between mental states—as shown by behavior and efficiency—and meteorological conditions, as the basis of a thesis for the Doctorate of Philosophy in this University. In addition to the compilation of a great number of data, I am attempting, by means of the enclosed questionnaire, to get the opinions of those who are in positions to make observations bearing upon the subject. If you are disposed to aid by answering its questions, the kindness would be greatly appreciated. If you have subordinates under your charge to whom you would be willing to distribute these blanks, use enclosed envelope for returning name, address and number of blanks desired, and I will send you a reprint of a paper already published by me on the subject, together with the blanks.

Thanking you in advance for any courtesy I may receive at your hands, I am,

Very respectfully,
EDWIN G. DEXTER,
Fellow in Education.

Date,..

Name,..

Position,..

1. About how many individuals are under your direct observation:
   Adult Male.................... Female....................
   Youth (15-20) Male.................... Female....................
   Children Male.................... Female....................
2. Have you noticed any seeming effect of weather conditions on their deportment?

	Hot.	Cold.	Windy.	Calm.	Stormy.	Muggy.	Cloudy.	Clear.
3. What kind of days seem to affect them for the worse?								
4. On what kind of days do they seem at their best?								
5. On what kind of days does mental work seem at its best?								
6. At its worst?								
7. If your charges do mechanical work, on what kind of days can they turn out most?								
8. Least?								

Put X in proper space. 'Hot' and 'Cold' mean for the time of year. 'Muggy' means 'sticky' or humid.

Extended remarks bearing upon the subject are very much desired, and may be written on the other side of this blank.

If you have noticed a different magnitude of effect upon men, women, boys or girls, please state it.

In many cases the request for extended remarks bearing upon the problem was complied with, and much valuable material was secured. The questionnaire is fully discussed in the chapter 'Deportment in the Public Schools,' 'Discipline in the Penitentiary,' and 'Insane,' with which data it particularly deals.

The classes of empirical data studied, together with the number of each and the way in which they were obtained, are as follows:

First.—Registration in certain of the public schools of New York for the years 1895–96, 118,860.

In this and the other classes of data which have to do with the New York schools the primary departments only were studied for three of the largest schools of the city, viz., Public School No. 93, corner of Amsterdam Avenue and 93d Street; Public School No. 43, corner of Amsterdam Avenue and 129th Street, and Public School No. 10, corner of 7th Avenue and 117th Street. The Primary Departments were studied first, with the intention of similarly considering the Boys' and Girls' Grammar Schools separately, for the purpose of making a comparison of the meteorological effects upon pupils of different ages, and especially upon the two sexes at the period of adolescence. Inability to procure the data desired without an immense amount of time devoted to a study of the records makes it impossible to include such a comparison in the present paper.

By the term 'Registration' is meant *expected attendance.* The school registers placed at my disposal through the kindness of Superintendent Jasper, of the New York public schools, show what is termed the 'Permanent Register' and also the 'Temporary Register.' In the former are included the names of all the pupils who have been registered in a given grade for the entire term. In the latter are included only the names of those who are for the day in regular standing in the grade, and expected to be present. The latter were taken as representing this class of data. The figures given indicate the number of pupils who were registered for a single day's attendance in the grades studied. I shall call, in succeeding discussions, the school attendance of a single pupil for a single day, one *pupil-day.*

Second.—Attendance in the same schools for the years 1895-96, 108,020.

The data were obtained from the records already alluded to, and the number indicates the number of pupil-days recorded for the exact conditions studied under registration.

Third.—Deportment in the same schools for the years 1895-96, 14,083. Under this head are tabulated for the years studied the number of pupil-days on which marks for imperfect deportments were given. The pupils are marked, when any record for deportment is kept, on a scale of ten, and any mark below ten is considered imperfect. It might at first seem that there would be some value in considering the various marks between ten and the lowest given, as indicating misdemeanors more or less grave; but upon noting certain individual peculiarities in the marking, it was decided that no sufficiently exact criterion could be had, and all not perfect were bunched as imperfect. For instance, some teachers never gave a mark lower than nine, while others invariably indicated imperfect deportment by marks from five to eight. As it cannot readily be supposed that all the pupils under the second teacher were so much worse han any under the first, we must conclude that the differences were in the method of marking, hence an introduction of error if used for exact purposes of comparison.

It might be added, in connection with our statements of data having to do with the New York public schools, that much time was spent in an ineffectual attempt to find records indicating the daily marking in the schools as showing the perfection of class work. No such records are required in the public schools of the city, and in a long series of inquiries not a single teacher was found who had imposed upon herself the task of keeping them. This necessitates the omission from this paper of an interesting phase of the problem, which I hope some time to supply from records of some of the older private institutions, which a diligent search may unearth. The data under classes twelve and thirteen have to do with the effect of meteorological conditions upon the ability to do mental work, and may in part take the place of the class here missing.

Fourth.—Deportment in the public schools of Denver, Colo., for the years 1882-96.

In the study of the public schools of Denver, Colo., made one year previous to the studies in New York City, the records for the fourteen years from 1882 to 1896 were made use of.

No record of daily deportment was kept for any part of that time, the only thing throwing any light upon the question being a record of corporal punishments administered. This record was in the form of notes written in compliance with a requirement of the school board to Superintendent Gove, stating the names of the pupils to which corporal punishment had been administered, and for what. Only those notes were used as data which stated the exact day on which the punishable misdemeanor occurred. It can readily be seen that the exact date of the misdemeanor, and not that of the punishment, is the important thing from our point of view, and many notes had to be discarded because of uncertainty on that point. No attempt was made at any classification of the misdemeanors recorded—so varied were they in their character—nor was the tabulation for the two sexes kept separate.

Fifth.—'Assault and battery' for the City of New York for the years 1891–97: Male, 36,627; female, 3,134.

These figures represent the total number of arrests for this crime for those years. They were taken from the records in the wonderfully complete archives of the Police Department of the city. The records for the two sexes were considered separately for purposes of comparison.

Sixth.—Discipline in the New York City Penitentiary for the years 1891–97, 3,981.

The record of dark-room punishments was made use of, it being the only record of deportment permanently kept at this institution. Each man's chance for commutation of sentence depends upon the freedom of his record from bad marks, and by looking over the individual's records the data were collected. Care was also taken to make certain that the misdemeanor occurred upon the same day as the recorded punishment.

Seventh.—Arrests for insanity for the city of New York for the years 1891–97: Male, 2,467; female, 1,097. These data were taken from the records at the Central Police Station, and represent the entire number of each sex who were arrested upon the streets by the police of the city, or were taken from their homes, mentally unbalanced. In most cases they were initial attacks of insanity or, at any rate, the beginning of a recurrent period.

I have visited most of the asylums in the vicinity of New York City in the hope that I might secure some record of discipline or restraint throwing light upon the daily deportment of the inmates, but as yet unsuccessfully. It may be that some future search will be productive of more results and that this phase of the problem may be completed.

Eighth.—Deaths for the city of New York for the years 1886–87, 74,793.

This includes deaths from all causes. The data were taken from the books of the Department of Public Health. The years 1886–87 were used, not that they were any more interesting than other years, but because they were the last for which records were kept in such a way that the exact date of death could be determined without some doubt. Beginning with 1888 the record of issuance of burial permits was the only one kept, and as these were sometimes issued on the day of death, but just as frequently on some following day, these latter records would not have been sufficiently exact for our purpose.

Ninth.—Suicides for the city of New York for the years 1886–87, 706.

These data were collected from two sources, viz., the records of the Department of Public Health and those of the Police Department. The record of successful suicides came from the former source, and are included in the death records; but, since for our purposes an unsuccessful attempt was as valid a datum as the successful, indicating, as it does, suicidal intent, they were included in this study. The police record was the source of our information on this subject, since suicide is considered a felony.

Tenth.—Suicides for the city of Denver, Colorado, for the years 1884–97, 260.

These data were taken from the voluminous scrap-books of newspaper clippings kept by the chief of the detective force of Denver. The city keeps no official register of crime, but the officer mentioned has kept a most complete one, clipped from the columns of the daily papers, which, through his kindness, was placed at my disposal. The data include both suicides and unsuccessful attempts.

Eleventh.—Murders for the city of Denver, Colo., for the years 1884–96, 184.

These data were taken from exactly the same source as the preceding. A record for assaults, such as is made use of in the study for New York City, would have given us many more data of a class indicative of an emotional state analogous, perhaps, to the homicidal; but it seemed probable that the newspaper record was not complete for this class of crimes, so the record of murder was taken as being more trustworthy and exact.

Twelfth.—Clerical errors discovered in the records of certain of the national banks of the city of New York for the years 1896–97, 3,698.

These data were taken from books known as 'Correction of Errors,' kindly loaned by some of the largest national banks in the city, among them, in fact, the largest bank in the country. These books show the data and the magnitude of all errors made by the employees of the bank, together with the date of their discovery and correction. The date of occurrence was, of course, the important item for our study.

Thirteenth.—Maximum strength tests taken in the Gymnasium, Columbia University, 3,000.

I had hoped to make a somewhat exhaustive study of the effects of meteorological conditions upon the ability to do mechanical work, but was unsuccessful in my attempt to secure the data. Nearly all of the large manufactories of pearl buttons, many of the extensive printing establishments and several cigar factories were visited, in the hope of finding some record of the daily productions by individuals, but none was found sufficiently exact to use as the basis of a study.

Fourteenth.—A study in discrimination carried on in the Psychological Laboratory of Columbia University, 50.

The empirical study of this paper, as was stated at the beginning of this chapter, is made up of a comparison between the occurrence of data under definite meteorological conditions and the prevalence of those conditions. Incidentally, also, is shown the distribution of the data for the months of the year, and in some cases for the days of the week.

Since all the conclusions of the paper are based upon what I have called 'expectancy,' and any interpretation of the chart presupposes a full understanding of this term, I shall now, at some length, explain its computation and application.

The first process in its construction was carried on at the New York office of the U. S. Weather Bureau (Denver Office for the Denver study). There, in a specially ruled blank-book, were copied the mean *Barometer, Temperature, and Humidity, the Total Movement of the Wind, the Character of the Day, and Precipitation* for every day of all the years for which any of the data of the studies were collected. For New York these years were 1886–87 and 1891–97, inclusive, besides the limited periods in the years 1898 and 1899 made use of in studying the classes of data already designated as XIII. and XIV.

For Denver the years so studied were those from 1883 to 1896 inclusive. Separate 'expectancy curves' were constructed, by the method I am about to explain, for the period 1886–87, the period 1891–97, the school months of the calendar year 1895–96 and for Denver.

Since these curves were all constructed in the same manner, a description of the process for one will answer for all the others, so I will explain those only for the 1891–97 period.

First.—The expectancy curve for temperature.

By a process of tabulation which was practically but a simplified method of counting, it was ascertained that of the 2,557 days of the seven years considered, 2 had been recorded as having a mean temperature between 0° and 5° Fahr.; 3 between 5° and 10°; 21 between 10° and 15°; 28 between 15° and 20°; 80 between 20° and 25°; 142 between 25° and 30°; 203 between 30° and 35°, and so on, until all the 2,557 days were thus accounted for; in some one of the groups of 5° temperature between zero, the lower limit, and 95°, the upper limit.

It would have been possible in a similar manner to count the number of days for each single degree; but a curve based upon such a complicated series would be almost unintelligible; so definite meteorological groups have been chosen for each condition, as being better for our purpose than the single unit of measure.

Having thus counted the number of days occurring in each one of the temperature groups of 5°, the next process is to turn these numbers into percentages of the whole number of days, and we find that one-tenth per cent. of the days for the seven years had a mean temperature between 0° and 5°; .2% between 5° and 10°; .8% between 10° and 15°; 1.1% between 15° and 20°; 3.1% between 20° and 25°; 5.5% between 25° and 30°, and so on. These figures represent the normal prevalence of the temperatures represented by each of the groups, and are given in full in the table of temperatures (page 85) under the heading "Normal."

Now, it can be readily seen that this represents the percentage of data of any class which the law of numerical probability would lead one to expect under that condition, *if the temperature exerted no influence*. For instance, if 5.5% of the days for the seven years had a mean temperature somewhere between 25° and 30°, the law of probability would lead us to expect that same percentage (5.5) of the entire number of murders or suicides or deaths occurring in that period to have occurred under that temperature group, *provided the temperature itself had no effect*.

To illustrate: One-seventh ($\frac{1}{7}$) of all the days of the year are Mondays, $\frac{1}{7}$ Tuesdays, $\frac{1}{7}$ Wednesdays, and so on through the week. Now since one-seventh equals 14.3%, 14.3% of the days of a year, or any number of years, are Mondays, 14.3% Tuesdays, and the same for the other five days. Since this is so, the law of numerical probability would lead us to expect that same per cent. (14.3) of all the murders or suicides or deaths for a series of years to occur on each of the days of the week, provided there was no condition, social or industrial, to affect their distribution. Whatever variation might be found to exist must be ascribed to some constant force affecting the conditions.

On the temperature table, then, as on all the tables shown, the line near the top marked 'Normal' represents the normal occurrence of the condition indicated in the top line, and also *the expected occurrence for each class of data.*

For the barometer expectancy, one-tenth ($\frac{1}{10}$) in. variation in the heights of the mercury column was taken as a unit of difference; for humidity, five-hundredths ($\frac{5}{100}$); and for wind, a difference of 50 miles in the total movement for the day. All these percentages may be found in the tables given (pp. 83–88) under the heading 'Normal,' and also plotted to a scale upon the charts under the heading 'Normal Prevalence.'

As has been stated, a special 'expectancy curve' was made use of for the year 1886–87 and for the school years. The necessity for so doing was this: Although the 'expectancy curve' for the seven years from 1891–97 inclusive may be considered a normal curve for all time, it could hardly be referred with exactness to isolated years without the introduction of considerable error, due to the variation of those years from the true normal, hence the necessity of extra labor in the preparation, in exactly the way outlined, of a special curve for these years.

A separate curve for the school year is even more necessary, since so large a portion of the calendar year, and that at one of the extremes of temperature, is left out. Neither of these special expectancy curves is shown upon the tables or charts, though they have been made use of in plotting all the curves indicating conditions for death, suicide and the school problem.

Having explained the construction of the 'expectancy curve,' it now remains to show its application. Opposite the meteorological conditions for each day, as they were copied in the blank-book already referred to, were placed, in separate columns, the number of data for each class for that day,—that is, one column for each of the data relating to the public schools, one each for male and female assaults, one for discipline in the penitentiary, one each for male and female insane, and so on for all the classes studied.

Since all were treated alike in constructing what I have called the 'occurrence curve,' that for male assault only, in its relation to the expectancy curve for temperature, already discussed, will be considered. The sum of the data for this class of assaults for the 2,557 days of the years 1891–97 inclusive was 36,627. By the process of tabulation made use of in constructing the expectancy curve, it was found that 50 of that number occurred on days when the temperature was between 0° and 5° Fahrenheit; 53 between 5° and 10°; 253 between 10° and 15°; 288 between 15° and 20°; and so on for each group of 5 degrees up to the upper limit of daily mean. Turning these numbers into percentages of the whole, as was done in the previous discussion, we have .1% for the temperature group 0° to 5°; .1% between 5° and 10°; .7% between 10° and 15°; .8% between 15° and 20°; as shown by the line of percentages in the temperature table (page 85) indicated by the heading 'Male Assault' in the left-hand column of this table. The curve itself is not shown upon any of the charts. Now, we have already computed and shown in the 'expectancy curve' the percentages which the law of numerical probability, applied to each of the temperature groups, would lead one to expect. A comparison of these percentages of occurrence with those of expectancy would indicate whether there was an excess or a deficiency for any of the groups. This is shown numerically in the second line of figures under 'Male Assault,' in the temperature table, and graphically upon the temperature chart, by the curve marked 'Male Assault.' To explain more fully, making use of the figures for the temperature groups from 0° to 15°.

The line marked 'Normal' on the temperature table shows that for the groups 0° to 5° we have an expectancy of .1%.

The line marked 'Male Assaults,' that there was an occurrence of .1%; the next line below, under 'Male Assault,' that there was neither excess nor deficiency, and is marked ± 0. Upon the temperature chart (Fig. 10) the fact is indicated by the curve marked 'Male Assault,' starting in this group (see left column) from the heavy horizontal line to which the relation between 'expectancy' and occurrence is referred in all the charts. From this table it will be seen that for the temperature group 5° to 10°, the expectancy was .2%, and the occurrence .1%. But .1% is but one-half of .2%, *or* 50% of .2%, *or* 50% less than .2%, and we have its relation to the expectancy indicated as −50%. This fact is shown on the temperature chart by the curve in this group being, as it is, two and one-half of the squares which represent 20% below the heavy line. For the group 10° to 15° the expectancy is .8%, and the occurrence .7%. But 7 is 1, or ⅛ of 8, less than 8, or 12.5% (= ⅛) of 8 less than 8, which fact is indicated upon both the table and the chart. For the next group the expectancy is 1.1% and the occurrence .8%. By the same computation it will be seen that .8% is .27% of 1.1% less than 1.1%, which is also indicated in both places.

This is the method of computation made use of in all the curves except those marked 'Normal Prevalence' (which is, in fact, the expectancy curve), and the 'Attendance' curve upon all the charts except those for data classed as XIII. and XIV.

In brief, when a curve is above the heavy horizontal line it indicates an excess of data to an amount represented by its distance above the horizontal or datum line; when below, a deficiency of the magnitude indicated by its distance below. In both cases the temperature group is shown by the figures at the top of the chart. Although the curves show with empirical exactness the relation between the *expected* number of occurrences and the actual number for any group of meteorological conditions, there are some few facts which might well be borne in mind in regard to them:

First.—They mean more and perhaps are more valid near the center of the curve than at the extremes, for the reason that near the center they are based upon more data and are, therefore, less liable to be affected by accidents. The normal 'expectancy curve' upon each plate shows the percentage of occurrences expected for each meteorological group, and from it the relative values of each of the other curves for those groups may be judged.

Second.—Each meteorological condition has to be studied and the curves constructed as if none of the other conditions were in any way effective.

Unless two or more of the conditions tended to vary in the same way, this fact would have no effect upon the curves, as otherwise, in the great number of days studied, the influence of one upon another would tend to negative the effect. If, however, two conditions generally accompany one another, there would be a possibility that effects indicated by the curve of one condition were contributed to by the other, without that fact being in any way shown.

Third.—The excesses and deficiencies indicated by any curve need not be and probably never are equal. Although the sum of all the percentages of the 'Normal Prevalence Curve' and also the 'Occurrence Curve' (not shown), upon which the curves for each class of data are based, is 100, it is not so for the curves plotted, because the latter are based upon many more data in some parts than in others. To give a concrete example: in a certain meteorological group 1% of all the data considered might be expected and 2% occur,—that is, 1% more, or twice as many,—but the plotted curve would show 100% excess. In another meteorological condition 10% of all the data might be expected and 9% occur (the same actual number less), while the plotted curve would indicate a deficiency of but 10%. From this illustration it may be seen that there can be no constant relation between the indicated excesses and deficiencies of any curve.

Fourth.—By making use of meteorological groups rather than smaller units of measure for each condition, some of the real effects of those conditions may not be indicated by the curves. It would, however, be utterly impracticable to work out the expectancy and occurrence for each degree of temperature or for each hundredth of an inch of the barometer, and I am inclined to think that the loss for grouping is very slight. Certainly there is no positive error introduced by so doing.

SECTION III.

THE METEOROLOGICAL CONDITIONS.

Since we have considered the effects of weather as but the resultant of the combined effects of its ever-varying components, it is necessary that those components be defined and a description given of the process of their measurement, as carried on by the United States Weather Bureau.

Incidentally, too, we mention the recognized influence of each meteorological condition upon climate, in its effects upon racial traits.

TEMPERATURE.—Man surpasses all his fellows in the animal kingdom in his ability to live in different degrees of temperature, the extreme range of the thermometer for all parts of his habitat being nearly 200 degrees Fahrenheit. It is even probable that a single individual might experience both of these extremes for a brief period without disastrous results. In their effects upon the race, however, varying temperatures have been recognized by every student of climatology. Inhabitants of hot climates are apt to be listless, uninventive, apathetic and improvident. An equable high temperature, especially if moist, weakens body and mind. No long-established lowland tropical people is a conquering race in the broadest sense of the word. For the inhabitants of the higher altitudes, even under the tropical sun, this may not be true; for as we ascend the temperature lessens about 1 degree every 270 feet on an average, and even at the equator we may have a temperate climate.

The most favorable temperature for health that carries with it an aggressive energy which is felt, and which has led the world-march of civilization, is about 55 degrees to 70 degrees, on an average; and this is found in the temperate zones. From there have come the brawn and brain of martial conquest and intellectual attainment. The dominant peoples are shown between the latitudes of 25 degrees and 55 degrees. Farther north the available vital energy seems so largely expended in furnishing mere body heat and stimulus for the necessary physiological functions that there is little left for use in those activities which make leaders.

The question as to the ability of races to thrive under conditions of temperature other than those of their ancestors is one which has received considerable attention. It has long been held that the tropics could never become a field of conquest for the nations of the temperate zones, since the climate rendered occupation by them impossible. Notwithstanding the fact that distinguished observers maintain this,[1] experience seems to demonstrate that acclimatization depends very largely upon a rigid observance of sanitary and hygienic rules, and many places which were once considered fatal to the white man are being proved comparatively healthful. When we consider that they have lost their bad name solely by an exercise of local and personal hygiene, we must not despair of the power of man to reduce the unhealthfulness of even large areas in tropical climates.

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[1] Pearson, Wiener, Orton, Curtis.

The apparatus used by the Weather Bureau for measuring the exact temperature at its various stations are the thermograph and the maximum and minimum thermometers. The former gives a continuous record of the temperature; the latter two show respectively the highest and the lowest for each twenty-four hours. The temperature readings made use of in this study (except for classes of data XIII. and XIV.) are the average of the maximum and minimum for each day. This is found to be practically the average of all the temperatures for that time—that is, of the thermograph readings.

BAROMETER.—At the sea-level, or near it, the mean barometer readings for the year are practically the same the world over. Any study, then, of the effects of climate as indicated by prevailing barometric conditions must be largely one of altitude. At the level of the sea the weight of the atmosphere above is equal to the weight of a column of mercury 76 mm. or 29.98 inches high, this being the normal height of the barometer at that level. As we go to higher altitudes, we climb through some of the atmosphere, leaving it at levels below us. This fact is shown by lessening of the height of the mercury column as compared with lower barometric readings. For the lower regions of the atmosphere (three miles or so) the mercury column falls about one inch for each 1,000 feet of ascent. The variations of the barometer at a given location are due to atmospheric conditions, such as moisture or an upward or downward direction of wind columns. We are not, however, concerned with such variations in this chapter.

The general effect of high altitudes is undoubtedly an invigorating one, though liable to affect disastrously the action of the heart and the nervous mechanism. The ability of a healthy man suddenly to transplant himself to altitudes at which the pressure of the atmosphere is reduced one-third or even more, without effect, seems wonderful. I have myself spent some weeks in succession at an altitude of over two miles, and on several occasions spent the night upon the summit of Pike's Peak (14,147 ft.), without noticing any effect other than a slight quickening of pulse and respiration. Prolonged residence, however, at any altitude above one mile seems, at least in Colorado, to prove cumulative in its effect upon the nervous system, inducing in most cases a neurotic condition taht is only relieved by a temporary residence in a lower altitude. These facts will be alluded to in a discussion of the Denver curves.

Dr. Marcet says,[1] "The effect of altitude and cold combined is to increase the amount of carbon dioxide expired. Less air is expired at high altitudes. It appears that the blood more readily acquires oxygen there than at low levels."

Increased expansion of the chest and action of the heart adds to the strength and vigor, and mountain races are generally fine in build. If, too, we are to judge by the histories of the Swiss people and of the Spartans, or even the mountain Indian tribes of our own country, compared with those of the plains, we must concede that boldness in the face of danger and a love of liberty are in some way closely associated with life in the higher altitudes.

For studying the pressure of the atmosphere, the stations of the Weather Bureau are equipped with self-registering aneroid baragraphs and mercurial barometers. Readings from both are corrected for the altitudes of the station, and the latter for variations in temperature. The barometric data used in this study (except for classes XIII., XIV.) are the ***means*** for the maximum and minimum of each day.

HUMIDITY.—Great humidities are preëminently characteristic of the climates of certain localities, and very low ones of others, and the effects of the two conditions upon the inhabitants have been the subject of considerable study. The greater part of it has been, however, to discover their effect upon various prevailing diseases, though not without bringing out incidentally the general influence upon race type. From the standpoint of health, dry air is almost universally favorable to human life. This may be due partly to the fact that the germs of contagious diseases do not there find the culture-media for propagation which the moist surfaces of rocks and foliage in a humid atmosphere present, although there is undoubtedly a direct organic effect as well. Excessive heat together with high humidity forms a most deadly combination for one not acclimated to it, as the mortality on the west coast of Africa testifies; while in some localities—as, for instance, western Ireland, the lake regions of England, and the extreme northwestern coast of our own country—much moisture from a great rainfall without excessive heat is not particularly unhealthful. In its effect upon the emotional and intellectual characteristics of a race, it is hard to consider humidity apart from the other weather conditions which accompany its peculiarities.

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[1] Proceedings of the Royal Society.

A region of high humidity must be one of much rainfall, fog and many cloudy days; while one of low humidity, in all probability, lacks these as prevailing conditions. As they are all considered under other headings of this section, and humidity has incidentally been touched upon in the previous topic, we shall not attempt it here.

The determination of humidity, or relative humidity, as it is called by the Weather Bureau, is accomplished by means of two thermometers, the so-called dry-bulb and the wet-bulb.

The first is the ordinary thermometer, by which the temperature is indicated. The second is similar, except that the mercury bulb is surrounded by a cloth which may be saturated with water. When this is done, air is artificially made to pass over the saturated cloth, which causes the water to evaporate rapidly. The heat which is rendered latent by this process comes from the mercury of the thermometer, making it descend in the tube with a rapidity proportional to that of evaporation. In dry air the latter process is much more rapid than in damp air, and by noting the difference of the readings of the dry-bulb and the wet-bulb instruments, and making a simple mathematical computation, the relative humidity is determined. It is read in hundredths, one hundred meaning air saturated with moisture, and zero, air free from it. The former point is sometimes reached; the latter never. The humidity made use of in this study (except XIII., XIV.) is the *mean* of a morning and an afternoon observation.

WIND.—As an element in climate, the effects of the wind are harder to determine than those of other meteorological conditions. Not that its effects are not great; but that they are occasional, and might perhaps more strictly be regarded as an element of weather than of climate. A study of the chapters that compare the wind effects at Denver with those at New York will convince one that they certainly are not in any way proportional to the velocity of the wind; but with the prevalence of other meteorological conditions, together with the wind; each, seemingly, strengthens the effect of the other. The direct physiological effects of winds of different humidities have been noticed.

Dr. Arthur Mitchell,[1] in a report read before the Meteorological Conferences, speaks of the effects of the east wind prevailing on the coast of England:

"Such winds, blowing over a moist surface, such for instance as that of the human body, tend to reduce the temperature of that surface to the temperature of evaporation, which is much below that of the air itself. In licking up the moisture—that is, causing its evaporation—a large amount of heat is rendered latent. This heat must be taken from something, and, in point of fact, our bodies are, and must be, almost its entire source. A cold and dry wind therefore cools the surface of our bodies, not only by enveloping them in a cool medium and warming itself by conduction at their expense—it does that of course—but, being dry as well as cold, it does it with less activity than though it were moist and cold, damp air being a better conductor than dry air. It is chiefly, however, by the other mode that dry, cold winds abstract heat from our bodies—that is, by using their heat in the conversion of moisture into vapor. The heat so used becomes latent, and is, for the time being, lost. It does not raise the temperature of the air in immediate contact with our bodies. The quantity of heat which our bodies lose is probably far from insignificant, and the loss cannot be sustained without involving extensive and important physiological actions, and without influencing the state of health. In feeble and delicate constitutions the resources of nature prove insufficient to meet the demands made upon them, and a condition of disease ensues. It does not seem improbable that prevailing winds of such characters might be considered as elements in climate, which might in the course of time produce even racial changes. Certainly the nurture of disease, especially if the disease prove to any extent mortal, would, in accordance with the laws of heredity, tend toward the introduction of physiological tendencies, which might even have their mental concomitants."

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[1] *Nature*, Vol. 30, p. 355.

Of occasional winds, some, such as the siroccos of northern Africa, are so baneful as also, it seems, to leave permanent results upon the characteristics of the people. In *The Living Age*[1] there is a description of the peculiar effects of winds sometimes prevalent at Buenos Ayres, South America. Sir Woodbine Parish, upon whose book the extract is based, says:

"To the north of the city lies a very marshy district, while to the southwest lies the great chain of the Andes, separated only by the dry plains of the Pampas, and according as the wind blows from one or the other of these directions the effects are very remarkable. By the time the north wind has reached the city it has become so overcharged with moisture that everything becomes instantly damp. The effects produced in the human body are a general lassitude and relaxation, opening the pores of the skin and inducing great liability to colds, sore throat and all consequences of checked perspiration. This damp wind of La Plata seems to affect the temper and disposition of the inhabitants. The irritability and ill humor it excites in them amount to little less than a temporary derangement of their moral faculties. It is a common thing for men among the better class to shut themselves up in their houses during its continuance, and lay aside all business till it has passed; while among the lower classes it is always remarked that cases of quarreling and bloodshed are more frequent during the north wind than at any other time. Even murderers are said to lay to it the blame of their foul deeds. No sooner, however, does the southwest wind, blowing from the dry and snowy summits of the Andes, set in, than health and comfort and peace are restored."

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[1] Vol. 17, p. 1118.

Disastrous to good conduct as these winds seem to be, they are hardly worse than some exactly their opposite—both in temperature and moisture—which are occasional visitors to the higher altitudes of Colorado. During the prevalence of such, the humidity is invariably excessively low, and in the dry air there seems to be set up, by the movement of the wind particles and the leaves and grasses set in motion by them, an electrical state which in some undetermined way wreaks havoc with the emotions. Its effect is, however, shown empirically by some of the curves.

The Signal Service makes use of two instruments in studying the wind,—a vane with automatic electrical indicator for showing its direction, and the anemometer for registering the velocity. Both of these are exposed to the unobstructed force of the wind, though connected electrically with the indicators in the office below. Both the instruments register upon a drum revolved by clock-work, and at any time the direction and velocity of the wind can be seen at a glance. The total movement for the days, or the number of miles blown for that period, is used in all our studies except

CHARACTER OF THE DAY.—This condition has no relation to rain or snow-fail; nor is fog indicated directly by any of its conditions. The terms 'Fair,' 'Partly Cloudy' and 'Cloudy,' refer only to the periods of sunshine for each day compared with the whole time from sunrise to sunset. If for eight-tenths or more of the latter period the sun is obscured, the day is characterized as 'Cloudy'; if four-, five-, six- or seven-tenths are obscured, as 'Partly Cloudy'; if less, 'Fair.'

Many opinions have been expressed, both in general literature and in purely scientific writings, as to the racial influence of these characteristics of climate. Italy has always been 'Sunny Italy,' and England 'Gloomy Britain,' and the supposed effects of the two conditions made the excuse for many differing traits of character. Dickens based many of his tragic climaxes upon his analysis of their influences, and many other writers have made stock of them. Yet, after all, characterization of any race must be based upon definite acts of individual members of that race; and a comparison of races and racial traits should be made only through a comparison of the conduct of their respective peoples. In such an inductive study we fail to corroborate the mass of existing opinion with respect to the effects of a 'sunny clime' or its opposite. It has been stated that "the excessive number of suicides for England is due to its gloomy climate" (Montesquieu); but data show that the number per 100,000 for England is less than that for any other important Européan country.[1] Vilemais maintains that "nine-tenths of the suicides occur in rainy or cloudy weather." For data in refutation of that statement, in New York City at least, for the period considered, I refer to the curve for suicides in New York, shown upon Fig. 14.

Another interesting fact bearing upon this point is shown on the same chart by the Denver suicide curve; and, although the percentage of fair days is three times greater than in New York City, the suicides run up as much for cloudy days as in the latter climate they run down.

Even at the risk of robbing the section in which suicides are especially discussed, I shall here insert a quotation from a very interesting paper that appeared in *Once a Week* (Vol. 19) without signature (the writer was evidently not a Scotchman):

"The idea that the prevalence of suicide in this country is due to our bad weather is precisely one of those hasty and illogical inferences which are characteristic of the Gallic mind. The constant gloom of bad weather ought to acquaint us so thoroughly with moods of depression that suicide would never occur to us. Look at Scotland, for instance, where suicides are rare. Why are they rare? Simply because a succession of Scotch Sundays has so accustomed the people to prolonged despondency that any sudden misfortune cannot sink their spirits any further. One has only to spend a dozen Sundays in Glasgow or Edinborough to become inoculated against suicide.

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[1] Morselli, Suicide.

So far from November fogs driving people to jump off Waterloo Bridge, they ought to train and educate the mind to bear any calamity. A man who has taught himself to eat prodigious quantities of opium feels scarcely any effect from other forms of intoxication. We can educate our mental susceptibilities as we can our muscles, and the more we educate them the more they will bear."

There are many truths beneath the jocular vein of this quotation, and the writer expressed more fact than perhaps he knew. A constant succession of gloomy days is certainly not conducive to emotional flights; but the sturdy determination and evenness of temperament of the English have without doubt been contributed to by them. The almost constantly sunny climate of Colorado has, I believe, even in the few years which it has been enjoyed by the white man, tended to produce a state of emotional inequilibrium which has shown itself in many ways, and has had its effects upon the political and social history of the region.

The apparatus used by the Weather Bureau for determining the character of the day is known as the 'sunshine recorder.' It consists of a thermometer, with its bulb blackened to increase the absorption of heat from direct sunlight, enclosed in a vacuum tube to prevent as far as possible the direct influence of the temperature of the surrounding air. The piece of apparatus is in such a position as to receive the sun's rays during the entire day. When the direct rays strike the blackened bulb the mercury suddenly rises and closes an electrical circuit, which causes a record to be made upon a revolving drum in the office below. When the sun is obscured the mercury drops and the circuit is broken. The computation in tenths of the possible sunshine hours is made by the weather official.

PRECIPITATION.—There is little to be said upon this condition of climate, in its effect upon race characteristics, which has not been touched upon under the preceding topic. By it is meant a fall of water in some of its physical states.

Regions where precipitation is great are generally less healthful than where the amount is less, though some exceptions to this have already been cited. Attention might here be called to the fact that the relation between cloudy days and those showing precipitation is not so constant as might at first seem probable. That is, a day on which a shower took place, even though the rain-fall was considerable, would be classed among the fair days, provided the whole period of cloudiness was less than three-tenths of the period from sunrise to sunset. Since, however, there are no climates where days of such a character form a constant element, they need not here be taken into consideration. Undoubtedly thunder-storms induce in many persons emotional states which seem to be productive of excesses in deportment; but we must remember that these excesses are caused by the electrical conditions or by a superstitious fear which cannot be controlled, and we should not attribute them to the precipitation. These are, however, elements of weather, rather than of climate in its broadest sense.

The latest device used by the Weather Bureau for measuring the precipitation consists of a hopper or scale-pan which is so constructed as to tip and empty itself, and at the same time make an electrical indication of the fact in the office below, for each one-hundredth of an inch precipitation. By counting these records upon the revolving drum the officers can tell the exact time, the rapidity and the amount of precipitation for each shower or period of rain.

In our study, a day is considered as having precipitation if the hopper of the instrument has emptied itself once. No consideration is taken of the amount beyond this.

SECTION IV.

THE PUBLIC SCHOOLS.

The phase of the weather problem discussed in this section was, at the beginning of the work, the only one anticipated. The whole study was undertaken from the standpoint of the teacher, with the hope of answering some of the puzzling questions which arise within his mind as to the seeming effects of these conditions on the cosmical environment. As the work proceeded, it broadened more and more, taking in other classes of data cognate to the subject, until those mentioned in the last section were considered, with the field scarcely entered. The study has become a fascinating one, and though sanguine of the validity—as far as inductive studies may be valid—of certain of the conclusions, it might be well to state what seem to be the exact scope and bearing of the problem.

1st.—It does not seem probable that we are dealing with the *immediate cause* of any of the occurrences studied. In the case of those considered in the present section, we are not supposing that a low condition of barometer and a low humidity were the direct causes of misdemeanor, but that under such conditions an impulse to be refractory, whatever the immediate cause of the impulse may be, could not be as well withstood as under certain other conditions. In other words, the meteorological conditions are the essential causes of certain general physiological or mental states, some of which seem to be fertile fields for the action of immediate causes which are, from the standpoint of this problem, accidental. To be concrete: on a certain morning Johnny could not have what he wanted for breakfast, and went to school with the sulks, with a consequent disastrous effect upon his deportment. Most certainly the disappointment at home had a causal relation to his demerit, and no excuse from the weather is sought. But if we take the record of 200 Johnnies for 600 different days, and find that on certain days more of them are out of sorts than on other days, we look for a constant condition which might be considered in some way the cause. We cannot suppose that bad breakfasts or whippings at home or the disappointments common to child life would bear this constant relation, so look for it elsewhere. Wherever found, it must be considered valid. But it must be some factor which would be a part of the environment of all the children similarly affected. We have sought for it in the varying conditions of weather, with what success is shown by the curves which form the basis of our discussion. Remember, then, that when we say that high temperatures *cause* an excess of suicides or any of the other occurrences studied, we mean it only in this secondary sense. This explanation must serve as an answer to many of the hypothetical explanations of conduct which were made in answer to the questions we are about to discuss; in fact, would apply to all the abnormal states which were accidental to individuals, whatever their character or immediate cause.

2d.—Although the study discussed in the present chapter is based upon the deportment of children as judged by the teacher, it is not at all certain that the emotional state of the teacher is not an important factor in the result. Indeed, it may be the teacher that we are studying more largely even than the pupil. This has been suggested in many of the notes I have received, and it seems evident, by comparing the curves from the children with those based wholly on the conduct of adults, that the latter are affected even more than the former. But even granting this, we are studying the effect of meteorological conditions upon mental states, and since the present relation of pupil and teacher must remain what it is, our conclusions are valid.

3d.—The effects upon different individuals cannot be supposed to be at all commensurate. There may be many whom weather conditions do not appreciably affect; but in any inductive study we seek general laws, and though they may not be true for any single individual, yet they are valid for them all as a class.

Many of the remarks by teachers in various parts of the country in response to the request at the bottom of the questionnaire (see page 6) are very valuable, and I regret that space does not permit the publication of more of them in full. Of the 200 questionnaires sent out, 86 bearing upon the public school problem were returned filled out in full. Several who did not care to answer definitely the questions given, wrote their general observations in a very helpful manner. The exact number of pupils upon whom the answers to the definite questions were based were as follows:

For climates similar to that of New York (Philadelphia, Boston, and the coast cities and towns):

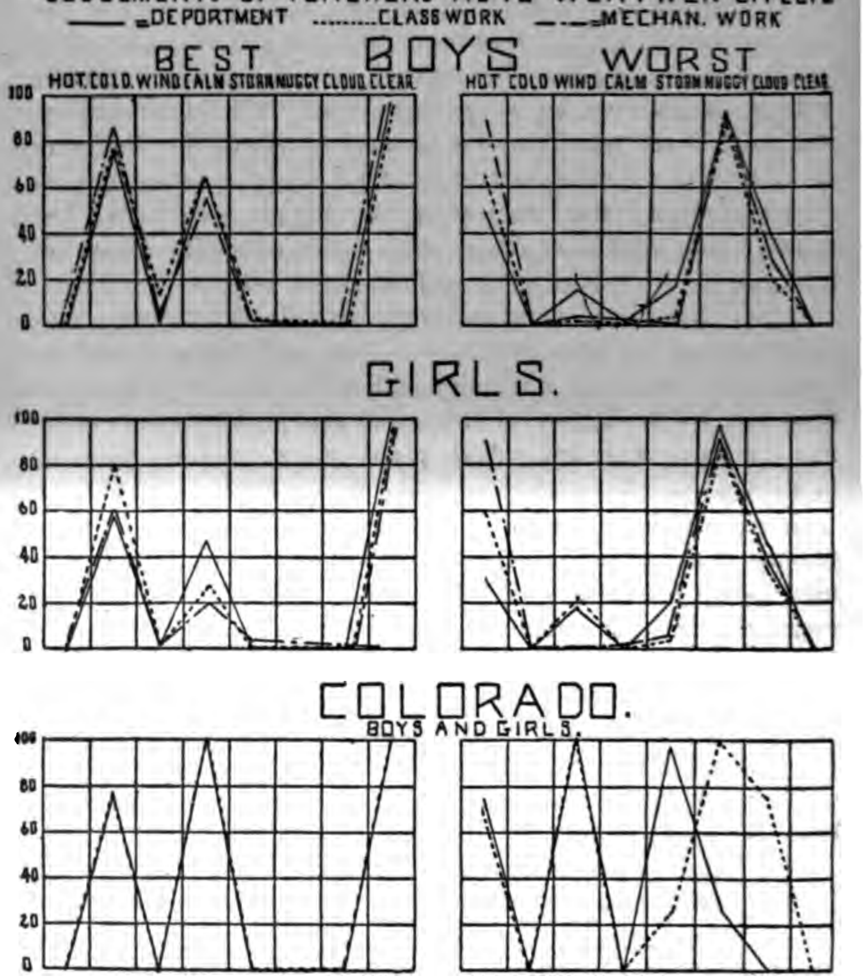
Boys	4801
Girls	3148
Boys doing mechanical work	3300
Girls " "	1500

For Colorado:
Boys and girls..................2218

For the latter climate no estimates were given for the production of mechanical work. In most cases the replies were from teachers of single grades, giving their judgment based upon observation of a limited number of pupils, though in some

FIG I.

JUDGEMENTS OF TEACHERS AS TO WEATHER EFFECTS



cases city superintendents and principals have sent me their estimates of the weather effects upon larger numbers—in one case (Boston), of 1700. The exact tabulation of all the returns is shown by the curves in Fig. 1. I have there indicated the judgments for the Colorado climate separately from those from other localities.

The curves show the percentages of all the children of each class for whom the judgments indicated were given. To prevent a complication of curves, I have separated those showing the conditions for which deportment or work was at its worst from those under which it was at its best. To interpret one of the curves in full: the entire line in the upper left-hand corner of the chart shows the judgment of conditions under which the deportment of the boys was at its best. None expressed the belief that it was so when the weather was hot. The judgments based upon 75% of the pupils observed were that they were at their best, as far as deportment is concerned, under conditions of cold. One per cent. believed the wind had a salutary effect, 64% calm, 1% stormy, none muggy or cloudy and 96% clear. All the curves may be interpreted in this way, remembering that the curves under the heading 'Worst' indicate the percentages of pupils upon which judgments for that state were based. The fact that the percentages shown by a single curve foot up to more than 100 indicates that more than one condition was mentioned as being that under which the pupil was at his best or worst. In most cases three conditions were thus mentioned, though the papers varied in this respect from one to as many as five.

It must, of course, be recognized that the judgment as to what constitutes a given condition—that is, 'Windy' or 'Muggy,' 'Hot' or 'Cold'—must vary with different individuals; but such conceptions are somewhat constant, and would probably not be the source of any considerable error.

Considering first the curves for climates similar to that of New York, it will be seen that cold, calm and clear days are those on which deportment and work are generally considered to be at their best, with the greatest unanimity with regard to the latter. For 'Worst' condition, muggy days take the lead, with hot and windy ones in second and third places. Comparing the Deportment curves for boys with that for girls, we see indicated what is referred to in the notes discussed farther on—that is, that boys are influenced more than girls by the weather conditions, at least by heat, cold and wind. Seventy-five per cent. of the latter (girls) were judged to be at their best on cold days and 64% on calm, to 56% and 46% respectively for the former (boys); while under conditions of heat 50% of the boys were thought to be at their worst to 30% of the girls. A fact worthy of note is the increased bad effect of heat upon the production of mechanical work, over its effect upon deportment or mental work. This would indicate that the increased metabolism of the processes of life under such conditions more largely deplete the reserve for motor output than for mental, and in spite of the irritating effect, emotionally, of the temper, leaves little energy for frolicsomeness.

To summarize in brief these curves, hot, cold, calm, muggy and clear days seem to be the effective ones; cold, calm and clear ones producing a favorable result, and the others the reverse. Windy, stormy and cloudy days are not generally mentioned as having much influence.

In discussing the curves for Colorado, we have but to call attention to a fact which corroborates in a very marked way conclusions arrived at later in this chapter, namely, the marked effect of the wind. All the returns state calmness as an accompaniment of desirable states in the pupil, and wind as the most disastrous. A muggy day is hardly known to Colorado teachers, so we get no corroborative evidence with regard to the problem of humidity treated later.

As shown by the notes received with the data which has been tabulated, I may say that there is a much greater unanimity of belief that the weather has its psychical effects, than of expressed opinion as to what those effects are or the meteorological conditions producing them. The influence upon the teacher of the conditions studied is not infrequently mentioned, and I quote a part of one of the letters bearing upon this point: “Make due allowance for my ‘personal equation.’ It is impossible for me to say how far my experience is subjective. It seems to be more marked in this matter than almost any with whom I have discussed it, and I strongly suspect subjective conditions. My experiences when in good and when in poor physical conditions correspond in kind, but are much more intense in the latter case."

The consensus of opinion, both as indicated by the curves and the personal notes, seems to be that girls are much less affected by weather conditions than boys. In eight of the notes the fact was alluded to, while none expressed the opposite opinion. One teacher, a supervising principal of elementary schools in one of the larger eastern cities, says: "The boys are very markedly more susceptible to weather changes than the girls. This apparent result may be due to the generally greater display of effects by the boy, who is under less disciplinary control than the girl."

And another, "Girls are greater adepts, not only at restraining impulses to do mischief, but also in concealing all evidences of it when it is in progress. This may be due to a greater horror on their part of an open reprimand."

It seems to me probable, however, that the matter of reserve and excess of vital energy enters into this problem. The preponderating anabolic tendency of the female, as opposed to the katabolism of the male, may be at the bottom of it.[1] In spite of this expressed belief of a less effect upon girls than boys, a few of the observations by principals, of the influence of the weather upon the teacher, state an opposite effect upon adults of the two sexes.

To quote from the principal of a large city school: "Men, it is true, are depressed on such days" (gloomy days); "but the average man keeps his nerve under proper restraint, which is something which the average woman, from the peculiar mechanism of her nervous machinery, finds it difficult to do. The writer has had during his thirty-four years of professional service the assistance of both sexes, and can say, fearless of contradiction, that men are better able to maintain that patience and quiet demeanor which are necessary in the school-room."

The relation of the two curves for males and females in the study of Assault is corroborative of this opinion.

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[1] See Evolution of Sex. Geddes and Thompson.

Considering the slight effect of the wind, as indicated by the curves upon the charts, a surprisingly large number of the notes make mention of this condition as having an adverse effect. This was expected from the Colorado returns—in which, indeed, it was universally mentioned as the most potent factor—but not in those from lower altitudes. Since, however, notes were appended to but a small proportion of the questionnaires, the more exact data expressed by the curves are probably more valid. The only other condition of weather which received more than a single mention was snow.

One teacher says, "A cold, snowy day, children restless and noisy." And another, "A day upon which there is snow seems (in case of boys) to bring all physical activity in them out, and makes them reach a high pitch of physical excitement." It seems to me possible that this is, in part at least, due to the anticipation of the active sports which snow makes possible. Coasting and snow-balling are always most attractive to the boy, and an impatience to be out of doors, even if the accompanying meteorological conditions were not directly effective, might bring about just the emotional conditions described.

Interesting as are their expressed opinions, from a scientific standpoint they cannot have the weight which the exact data must carry. Both, to be sure, are based upon the judgment of the teacher; but in the tabulation of demerits we have nearly 100,000 immediate judgments, while in a discussion of the opinions expressed in the notes we have but a few, and they liable to all the errors of introspection.

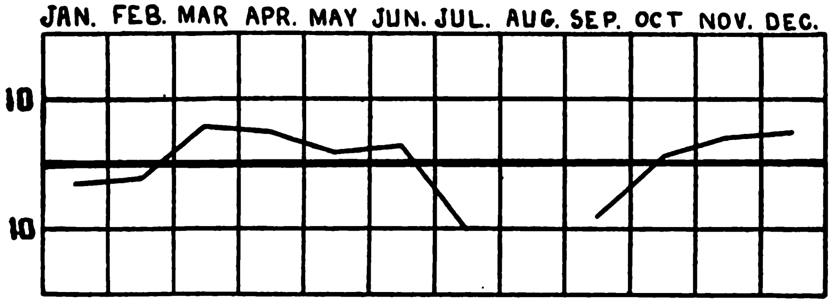
The number of data considered in connection with the study of school children is by far the greatest of any class studied. The labor, too, of securing them was the most difficult, as the records were of such a character that many volumes had to be gone over in order to get the required information.

REGISTRATION.—This term, as made use of here, is fully defined in Section II. so we can proceed immediately to its consideration. Of the entire number of 118,860 days registration, the distribution by months was as follows:—

JAN.	FEB.	MARCH	APRIL	MAY	JUNE	JULY	AUG.	SEPT.	OCT.	NOV.	DEC.
9723,	8811,	10,063,	10,041,	9963,	9833,	915,		7117,	11,149,	9816,	8455

The difference in numbers is largely due to the varying number of days in the different school months, though not wholly, as is shown by Fig. 2, in which the relation is shown between the *expected* and the actual registration for each month. The *expectancy* for each month was taken as that percentage of the whole number of registrations equivalent to the percentage

FIG. 2.



of the school days of the year, occurring in the month. This shows that for the schools studied the greatest registration is in the early spring, and again in the fall and winter to the Christmas recess; while it falls during the months of January and February and at the end and the beginning of the school year. Although these studies were made for the large city schools, the conditions shown are practically those observed by every teacher. The sudden drop for May is perhaps not readily accounted for; but the departure for the country and the sea-shore and the late returning in the fall make plain those for July and September. The weather alone is probably accountable for the deficient registration in the winter only.

Attendance.—Each of the charts of meteorological conditions has an attendance curve, which is not computed as are the others, except normal prevalence, with reference to one of expectancy. By the attendance curve the actual percentages of the pupils absent under all the conditions are indicated. This was done with the thought that this fact might be of more interest to the reader, especially if he be a teacher, than other values, which would be purely relative.

Occurrence.—From this curve it will be seen that there was a large number of absences for January (10.7%), a gradually lessening number through April (7.5%), an increase for May (8.8%), and the maximum for the school year in June.

Beginning in September with a considerable number (9.6%), the minimum for the year is in October (5.4%), and about the same for the next two months as for April and May. There seems to be nothing at all peculiar about the curve; in fact, it shows just about what the experience of a teacher would lead him to expect. When school-work is well under way in the early fall it is, I believe, always more carefully attended to by the pupil than at any other time of the year. Late fall and early winter undoubtedly present more climatic difficulties to attendance than there are in late spring; but as laxness in attendance has not yet developed, the showing is about the same. The bad weather of winter enforces irregularity on the part of many, and this factor, once introduced, tends to stick for the rest of the year, even though excuses because of weather conditions are not so valid. This curve shows for the very end of the year the same irregularity that was indicated by the registration. In the latter case staying at home was officially sanctioned; in the showing for this class of data it was not.

Temperature.—The attendance curves, in their relation to the various meteorological conditions, can be considered as throwing some light upon the effects of those conditions upon the health of the pupils. We cannot suppose that their prevalence, unless it be that of precipitation and perhaps very low temperature, presents any difficulties to school attendance, unless it be through inducing a state of ill health. We cannot suppose a parent would keep a child at home simply because the barometer was low or the humidity high, even if those facts were known. There are many good reasons, to be sure, why a child should be kept at home on a certain day, and it might happen that that day was one of peculiar meteorological conditions; but we have a right to suppose that for the 108,020 pupil-days considered such accidental coincidences would correct each other's effects. If, then, there be any noticeable relations between fluctuations in attendance and in meteorological conditions, the inference seems valid that the conditions were attended by physical indisposition or a state of low vitality on the part of the child. The temperature curve (Fig. 10) shows that there is a fluctuation in attendance with respect to the conditions indicated. The maximum number of absentees (15%) is shown for the lowest temperature group. As has been suggested, this perhaps indicates no prevailing condition of ill-health, but an unwillingness on the part of the parent to trust the child out of doors in such intense cold. The decrease, however, for temperature above a daily mean of 60° may indicate the relaxing effect of great heat, which is fully recognized and plainly shown by curves for other classes of data. It seems improbable that parents would fear the effects of temperature of the group 65° to 70° upon a healthy child, and thus keep him at home; yet under that condition we have the second largest percentage of absentees. This is an interesting fact when we consider that those unseasonably hot days of spring and fall, which will be shown to have such a marked effect upon the crime of Assault, are of the character indicated by this group. There seems little doubt that they lessened the vitality of the child, and that he was at home being nursed by a fond mamma. The maximum of attendance is indicated by the groups 20° to 25°.

It is very likely that fluctuations in some of the curves may be caused by accidental conditions which cannot, in this study, be discovered, and that only general tendencies in a curve have any value; yet when we note that the most bracing days of our winter climate, those days when the cold is invigorating without being intense, when the streets are dry even though there be snow upon the ground, are of just this character, we are inclined to doubt whether accident be the sole cause of the full school-room.

Barometer.—An inspection of this curve (Fig. 11) discloses the fact that attendance was relatively poor during both extremes of the barometric register, and at its best when the barometer was little above its normal mean. Its fluctuations for the groups between 29.40 and 29.60, so far as I am able to judge, are accidental in their origin. Reference to the chart of Monthly Means (Fig. 8) shows us that the barometer is normally high for the fall and winter months, and low for those of the rest of the year. There is here no constant relation shown between high and low barometric readings, and the barometer for those months of the year in which attendance was greatest, by which the peculiarities of this curve can be accounted for; and our conclusion must be that the barometric conditions themselves, or the meteorological influences which vary with them, affected the children in such a way as to make it impossible for them to attend. As the barometer is usually low during storms, it may be the latter which were really the effective agents, though showing upon this curve because of the coincidence of conditions. This fact, however, would not effect a decrease during conditions of high barometer, though the fact that such conditions not infrequently exist for a period immediately preceding bad storms might do so. It is a well-known fact that for some little time before storms of unusual violence, sometimes as much as forty-eight hours, a condition exists which strongly affects many people both physiologically and mentally. Persons afflicted with rheumatism and gout, and even those troubled with corns, seem to have intimation through the affected parts of the approaching atmospheric disturbances, and it does not seem improbable that children may in some way be influenced.

The probability seems to be that the very condition of the atmosphere—that is, its relative weight—is not the affecting agent, but other meteorological disturbances which vary with such conditions. The entire variation of the barometer for New York is but little over one inch, while in going from the level of the sea to Denver there is a fall of more than five inches, and to the top of Pike's Peak six more, without any excessive physical or mental abnormalities being evinced; and it does not seem reasonable to suppose that variations of a few tenths of an inch at the former place should be productive of results from the mere difference in the weight or density of the atmosphere.

Humidity.—This curve (Fig. 12) also shows accompanying variation in school attendance. Disregarding slight fluctuations, we have twice the number of absentees when humidities are very low or very high, as for certain intermediate conditions. It is not hard to account for the decrease in attendance for the higher readings, as they invariably accompany precipitation. Besides this, the de-energizing effects of high humidities accompanied by high temperatures are recognized; although the fact fails to show itself fully, as illustrated by certain curves discussed in the section treating of the death-rate. If, however, the combination has influenced this curve, it would be through conditions of health, indicating that the children were at home suffering from ailments due to the low conditions of vitality induced. The decrease in attendance indicated for the lower humidities is not easily accounted for by any regularly accompanying conditions of the weather which would present physical difficulties to getting out. Storms are rarely known for such conditions. We cannot, however, doubt an adverse effect, for the other curves upon the chart indicate an excess of abnormalities in conduct. This matter will be taken up with more detail in the discussion of the curves for Denver, which show such peculiar results for abnormally low humidities; but there seems little doubt that the uniformly increased electrical potential of dry air is the cause.

It has been demonstrated[1] that for every point upon the surface of the earth there are lines of electrical force radiating outward into space, generally negative in quality at the surface, gradually decreasing in potential and terminating in an electrification of the other quality at some unknown distance in space. It is also a well-known fact to scientists that the potential of these lines of force is much greater when the air is dry than when it is moist; when the humidity is great practically disappearing, but when it is low attaining a great magnitude. The exact physiological effects of this highly electrified state of the atmosphere have not all been determined, but it is certain that it induces a state of nervous tension which any one who has lived in an excessively dry climate cannot have failed to notice. Upon the arid plains of some of our more elevated Western States, Colorado, Wyoming and Utah especially, it is a factor which affects the emotional condition of the people in no small way, and undoubtedly has brought about the excessive rise in the curves for the Denver data, as shown upon this chart. Although the electrification is unusually less for cities than for the country, and is never excessive for climates as humid as that of New York, yet it may have had its effects upon the curve under consideration. If so, it would indicate that the children were at home suffering from nervous conditions which seemed to the parents to warrant missing school for the day.

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[1] See Smithsonian Report, 1895, p. 90 et seq.

Wind.—The attendance curve (Fig. 13) for this atmospheric condition shows no important variations except for very low and very high movements. The latter is easy to account for on the supposition that in a hurricane—such as a movement above 700 miles for the day—the children were kept at home because of the physical difficulty of getting about. It does not seem probable, however, that the decreased number for days on which there existed a virtual calm can be thus accounted for. This, it seems to me, must be an indication of disastrous effect of the condition upon health.

With regard to the effect of calms upon health, so far as I know, the only thing published is by F. A. R. Russel, Vice-President of the Royal Meteorological Society,[1] in which he states the results of a study made of the relation between death rate in London and the prevalence of low wind velocities. The conclusion of his study, briefly stated, is that the death rate for that city is considerably larger during months having many calm hours than for the months next following; or for the same month in other years when the number of calm hours is materially less. To give an example from his figures of the period from November, 1872, till December, 1893: "On the whole, the mortality is greater for calm than for windy weather, and there is much less variation in the death rate during the prevalence of strong wind than during the prevalence of gentle winds and calms."

Although these figures are for the death rate, and that is in no sense a factor in our study of school attendance, still it is an element which varies with conditions of health, and may be taken as an indication of its state. It has also been shown that during conditions of calm the number of disease germs in the air is much greater, especially in large cities, than under other conditions; but since no known germ produces sickness upon the same day it is taken into the system, it need not here be taken into consideration.

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[1] Smithsonian Report, 1895, p. 291

Character of the Day.—We cannot be certain that more is indicated by this curve than the direct effect upon attendance of the physical difficulties of getting about during storms. Possibly the difference between the attendance on fair and on partly cloudy days is due in part to fear of a storm on the part of the parent, as indicated by the weather conditions; although days characterized under the latter head might be stormy at the beginning of the session, and hence prevent attendance. The exact differences between days of the three characters may be seen from the table on page 85.

Precipitation.—Nothing more need be said under this head than is included in the preceding.

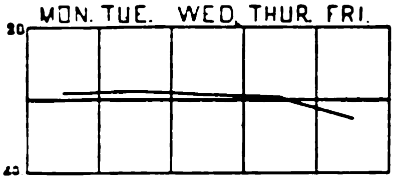
DEPORTMENT.—The attempt to discover the effect of the weather upon the children of the public schools, as shown by their deportment, was the first phase of the present problem undertaken. Indeed, in its inception it was the only study anticipated. As the work went on, however, the field broadened, and, one after another, interesting subjects of a cognate character presented themselves, until the various classes of data mentioned in Section II. were included.

The general opinions of members of the teaching profession upon the subject have been treated in the earlier pages of this section, so we shall proceed at once to an inductive study of the data presented.

Occurrence.—In the peculiar fluctuations of this curve (Fig. 9) we unquestionably have more strongly shown evidences of the force of other conditions than the meteorological. Certainly, general tendencies which might be pointed out are due to the latter; but the peculiar ups and downs are largely due to practices and customs associated with school management. A glance at the curve shows a somewhat gradual increase in the number of demerits (it seems probable that the excess for February is accidental) from the beginning of the year through June, in which month a very marked drop takes place. Again, at the beginning of the school year in September we have the deficiency still greater, with practically an equal number for October, November and December. The regular increase toward the hotter months is in accord with the showing made by other classes of data, and is undoubtedly due to the gradually increasing temperature; but the deficiencies for July and September are not to be accounted for in that way. Were it not for the fact that all the conditions under deportment are studied with reference to attendance rather than registration, we might conclude that the small number of demerits given was due to a small attendance; but since the small number of school days for these months are taken into consideration we find no solution on that ground.

The explanation which seems to me most probable, and which will, I think, appeal to all teachers on careful thought, is one of the standard of discipline set for different parts of the school year. At the beginning it takes some little time for the routine to become rigid, and a laxness is allowed which would not be tolerated during other months. I believe also that at the end there is a like decrescendo in severity. A teacher recognizes the fact that in the few days remaining a misdemeanor cannot become habitual, and rather than end the year with trouble she paves her path, and that of her pupils, with roses, by overlooking many things. Both of these tendencies would lessen the number of demerits, and we here have indications of the fact. It is not certain that the slight excess in the number of demerits for the last three months of the calendar year is brought about by the meteorological conditions. The heavy horizontal expectancy curve is in a sense an average for all the occurrences studied, and any condition forcing the curve either up or down must be compensated for by an opposite showing in some other point. The excesses for these three months may be due to the fact that the normal, or expectancy, has been so af-

FIG. 8



fected by the forced deficiency for the beginning and end of the school year as to leave the curve elevated at the other points, with the noted results. This factor of compensation is one common to all the curves.

The occurrence of demerits for the days of the school week is shown upon the chart (Fig. 3). It indicates a deficiency of 5% for Friday and a compensating excess distributed quite evenly for the other days of the week, though a little the greatest for Tuesday. Somewhat surprised at the fact that Friday should have been such a perfect day in deportment, I studied the data a little more closely, and discovered that it was brought about by the dominating influence of one school from which about 80% of the data had been collected. The other three schools studied showed Friday to have been the bad day of the week, but the excessive 'goodness' of the children of this school had entirely negatived the result. Upon questioning the principal of the larger school with regard to Friday's program and a possible explanation, an interesting solution of the mystery was disclosed. The principal said that good order was maintained largely through an emulation on the part of the pupils to secure honorable mention at the close of Friday's session for the best deportment in the room for the week. Also, that the pupils were conscious that the teachers were on that day considering the honor roll for the week, and it seemed probable that they were all doing their best in the hope that, because of their perfection for that day, misdeeds of the earlier days of the week would be overlooked and so the coveted prize won. It was also stated that the school session was one hour shorter on Friday than on any other school day. Both of these causes would have their effects upon the curve, though the latter would have been equally potent for any of the schools studied.

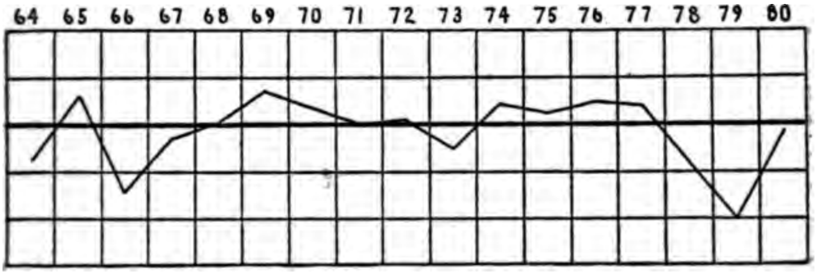
Occurrence: DENVER.—The monthly occurrence curve for Denver shows all the peculiarities of that for New York, except that of increase with the approach of the heated season. The school year is considerably shorter, practically closing with May, instead of extending into July, though it begins somewhat earlier in September. We note the same excess for February, which in the case of the curve for New York was attributed to accident. Coincidences of this sort would argue definite causes which were common to both places; but what they really are it is not easy to see. For Denver the wind, which is high for the month, might be a factor; but if so, we should expect a still greater effect for the month of March, when it is still higher. This, however, is not indicated. The lack of increase in the number of misdemeanors for the heated months tallies to an extent with the showing of the temperature curve, discussed in the next paragraph. There we see that high temperature indicates a deficiency of the data considered.

Temperature.—The seeming effect of temperature upon the children in the public schools both of New York and Denver, as shown by their deportment, is different in some respects from its effect upon any of the classes of adults studied. I believe, however, it can be shown that the conditions under which we are studying them are sufficiently unlike to account for at least a great part of the differences in their curve. Reference to the temperature chart (Fig. 10) shows a less number of demerits for temperatures below 45°, an excess for the groups up to 65° and again a deficiency for those which are higher. The lessened number for all the low degrees is fully in accordance with all the results of studies of the effect of cold climates upon physical and mental activities, and is suggested by the occurrence curve, already discussed. It is a well-recognized fact that under conditions of low temperature so large a portion of the vital energy is consumed in keeping the body sufficiently warm to allow a continuance of its metabolic processes, that little is available for deportmental excesses, and a condition of comparative lethargy exists. With an increasing warmth in the atmosphere this energy is diverted into other channels, and its expenditure is made known in more conspicuous ways. Nearly all the curves upon the temperature chart show this fact. Temperatures above 65° show the opposite effect upon the school children only, by decreasing either the energy at command or the irritability which shows itself in bad conduct. Upon noting the material deficiency of deportmental excesses under very high temperature both for New York and Denver—for the latter place to the extent of

300%—it occurred to me that it might be due to a difference between the temperatures in the school-rooms and those recorded at the Weather Bureau.

The school buildings of the city are large and well ventilated, and even in summer are comparatively cool, so it seems quite probable that the contrast on a summer day with the excessive heat without might have aquieting effect, which would be noticeable in the curves. The fact that the temperature of each school-room is taken three times during each day's session, once at 10:30 A. M., once at 1:30 P. M., the beginning of the afternoon period, and again at 3:30; and that these readings were recorded in the registers placed at my disposal, made possible a study which has thrown some light upon the questions. The curve in Fig. 4 gives the result. In this curve, as in all the others, expectancy was computed by reference to the number of times the different temperatures, indicated at the top, occurred. It will be noticed that most of the marked fluctuations of the curve are for temperatures for which the number is odd rather than even. This is undoubtedly brought about by the small number of data recorded under such conditions, due to the fact that the thermometer scale is divided into units of two degrees each, and the great majority of the teachers recorded the temperature by the nearest even number. There is a great lack of regularity in the curve; but if it shows anything, it is that for the highest temperatures recorded, which in fact run up only to 80°, there is a deficiency of demerits, while for those between 77° and 69° (with the one exception of 73°, which is an

FIG. 4.



odd number) there is an excess, and for those below 68°, with one exception, there is again a deficiency. I do not consider this curve an especially valuable one, yet its indications are somewhat interesting because of their similarity to those of the temperature curve for the year—that is, an excess only for moderately high temperatures. From this fact I would conclude that it was not wholly, if at all, the contrasting coolness of the school-room which kept the deportment good for the excessively hot day, but a relaxing effect, producing a low state of vitality which did not furnish sufficient energy to force an output in frolicsomeness. This last is true both for unusually high temperatures in the school-room and for the general meteorological condition; so much so for the latter, and so markedly in contrast with the curves for adults, as to lead one to conclude unquestionably that this effect of great heat is much greater upon the child than upon the adult. Whatever relation may exist between them in the quantity of energy each may have in reserve above what may be demanded under ordinary conditions, certain it is that in periods of excessive heat the surplus of the child seems to be exhausted much more quickly. This fact is borne out by the more rapid increase in the death rate of small children than of adults under high temperature, and, I believe, by our common observations in the matter.

Temperature: DENVER.—In considering any of the classes of data for Denver, it must be borne in mind that the numbers are much less, because of the smaller population of the city, and on this account accidental influences are much more liable to be effective and thus bring irregularities into the curves. We shall consider then that only general tendencies are valid, disregarding, for the most part, minor fluctuations. Attention should also be called to the larger meteorological groups made use of for most of the conditions. In the study of temperature, for instance, in New York, comparisons were made for each group of 5°, while for Denver 10° were taken as a unit. This is the case simply because the study for the latter city was the earlier one, and the larger groups made use of with the thought that they were exact enough. In attempting the more elaborate study for New York it was thought best to divide the groups, even though the labor was thereby nearly doubled. Upon the charts the heavier of the vertical lines indicate the limits of the Denver groups.

Upon inspection of the curve under consideration it will be seen that there is a general resemblance to the same curve for New York, though with an intensification of the latter's results. The great excess for conditions of very low temperature is not valid, it seems to me, from the standpoint of meteorological effect: first, from the fact that the number of data for such temperatures is so limited as to make them very easily influenced by accident; and second, from the character of the data studied in Denver such accident is very probable. Those data, it will be remembered (see Section II.), were not the giving of demerits for bad deportment during the hours of the session, but the administration of corporal punishment for some misdemeanor of considerable magnitude. Misdeeds quite as frequently occurred about the school building before and after the session as during its period, and in the winter months snow-balling and the accidents connected with it were very prolific sources of the youngster's tribulations. As this pastime could only be indulged in under the condition of temperature indicated by the lower part of the curves, and as there is not, perhaps, anything quite so seductive for other temperatures, its effect may have been considerable.

If we are correct in inferring that this is a factor, we cannot assert that it accounts for the entire excess in cold snaps. It does not seem impossible that the disregard for anything else, which we are all conscious of when aching with cold, may have introduced conditions of disorder which could not be overlooked by the teacher, and so the number of castigations were augmented. The decrease under conditions of great heat may be accounted for in the same manner as the similar showing for New York, already discussed—that is, a contrasting coolness of the school-room with the atmosphere without, or the devitalizing effect of high temperatures, or both. The study of the temperatures of the New York school-rooms would lead us to believe the latter effect to be the more powerful, reducing the number of misdemeanors, under such conditions, three-fourths (300%).

Barometer.—The effect of those conditions of the atmosphere revealed by different readings of the barometer seems to be somewhat constant for all the classes of data (Fig. 11). With the sole exception of the study of errors made by clerks in banks, that effect seems to be one of increase of all the occurrences for conditions of low barometer and a corresponding decrease for readings above the normal mean. Notwithstanding the fact that the pressure of the atmosphere, varying as it does for a given locality, has been considered by many a great force in disturbing the mental and physical equilibrium of the people, I believe, as has already been stated, that it does so largely through its production of other states of weather, which are themselves the efficient cause. This is not true for the other meteorological conditions, as temperature, humidity and wind have their effect *per se*. As stated under the discussion of attendance, from the standpoint of our present study the barometer is interesting only in its relation to other conditions of weather which themselves are influential. Those upon which its variations have a causal effect are the character of the day and precipitation, and this relation is very interesting. Conditions of low barometer are usually the accompaniment of storms, although a careful study of this relation for several years shows that when a storm is of several days' duration the period of depression exists only for a short portion of that time. That is, although it is practically true that all days on which the barometer is low are stormy days, not all stormy days are those of low barometer. Now, although the curve under consideration shows an excess of demerits for low barometers, those for character of the day and precipitation (considered in full later) show a deficiency of them both for cloudy days and for those on which there is rain or snow. Our conclusion from this must be, then, that the demerits which have brought the curve above the expectancy for low barometers must have been given on the particular stormy days which presented those conditions, while stormy days when the barometer was higher were sufficiently free from demerits to force their average below the expectancy and give us a deficiency for the condition as a whole. The general tendency of this curve for the school children compared with the others, all of which are for adults, would lead us to believe that the effect of the condition, whether immediate or secondary, is less upon children than upon adults. We must, however, take into consideration the fact that all these conclusions, which we are imputing to the effects of weather upon the children, may be little more than their effects upon their teacher.

The award of demerits is, of course, made in accordance with the judgment of the teacher, and a fluctuating criterion or a judgment warped by subjective emotional abnormality would give us the same results as a valid judgment of an affected object.

Barometer: DENVER.—Low barometers bear the same relation to the occurrence of misdemeanors for the climate of Denver that we have noted for New York, except that the effect seems to be intensified. For high readings it seems to be reversed, and at that end of the curve we again have an excess. The conditions of atmospheric pressure are, however, very different for the two places, for we must, in reading the exact height of the mercurial column for Denver, subtract five inches from the readings given at the top of each column. There seems to be nothing new to be said with respect to this curve.

Humidity.—The general tendency of this curve (Fig. 12) is to indicate an excess of demerits for conditions of low humidity, compared with the numbers shown for a moister air. Comparing it with the curves of the other classes of data, in which an emotional state affecting conduct can be considered a factor (Assault, Penitentiary and Insane), we find this tendency less marked for the children than for the adults. Judging from the answers to the syllabi with respect to the effects of 'muggy' and 'sticky' weather, I am inclined to think that this showing will be something of a surprise. The curves mentioned resemble each other so closely in their general tendencies that when we consider that nearly 100,000 data were made use of in their construction, we can hardly doubt their validity. Of the special curve under consideration, the marked fluctuations for both the end groups may or may not be due to accident. That of the group 45–50 is much more liable to be so than the one at the other extreme, for, as we see by noting the normal prevalence curve, less data were considered in computing its position than for the latter. The curve coincides wholly with the condition which we find for character of the day and precipitation, as all these show deficient numbers of data for conditions of moisture. Our inference from this curve must be, that excessive restlessness, together with those activities of mind and body which in the judgment of the teacher constitute disorder, increase with increasing dryness. Yet a dry atmosphere is recognized the world over as a vitalizing one, having the bracing properties which we all recognize in certain characteristics of weather. A logical conclusion for the two conditions stated is that bracing conditions of atmosphere and an excess of activity accompany one another, or, to carry the logical process a little further, that those activities depend upon and are the result of excessive vitality.

In arriving at conclusions thus, by an inductive process, we are not, of course, supposing that every individual of the aggregate is subject to this law. There may be many whom such a vitalizing effect would rouse only from a chronic state of physical and mental lethargy to a condition common to the rest under less stimulating conditions; but for the pupils as a whole the conclusions are valid. Interviews which I have had with many teachers upon the subject have tended to corroborate the view taken. They say that two tendencies on the part of the pupil are to be combated—inattention and roguishness; and, however differing might be the views of individuals with regard to the distribution of the latter, the former was more prevalent, they thought, on stormy and wet days. The present study would tend to prove roguishness more common when the weather is dry and clear. This is noticeable when we consider that lapses of attention would not be so apt to influence the giving of demerits as conduct that was objectively bad. The sins recorded there are those of commission rather than of omission, as inattention would affect the class-standing rather than the record of deportment.

The cause of the exhilarating effect of a dry atmosphere seems to be the increased electrical condition accompanying it; but since this is much more markedly shown for the climate of Denver we shall discuss it in the next paragraph. The sudden increase in the number of demerits awarded for the highest group of humidities is not wholly due to accident. It seems to me probable that we may attribute it to an utter lack of the power to inhibit, brought about by the relaxing effects of such an atmosphere. We have all recognized, I think, in ourselves or in others, a mental state brought about by weakening and relaxing influences in which any impulse was followed by its motor realization simply through an inability to control. The threshold of such a condition seems to be fairly well definable and not at all gradual. It comes after those stages of relaxation in which there is scarcely enough energy left to initiate activity, and generally shows itself in a condition of excessive nervous irritability. Although the other curves for data of this nature do not show it, and our conclusions may not be well grounded, still the possibility is, I believe, worthy of mention.

Humidity: DENVER.—The curves on the humidity chart make some of the most startling disclosures of the whole study. Whereas for New York we have slight excesses of the occurrences at low humidities, here we have for all classes of data excesses which seem almost incredible. For the school children it amounts to 400%, and for Suicide and Murder very much more. When we consider that this means that for the fourteen years studied the days on which the humidity showed a mean of below 30 had four times as many punishable misdemeanors as the law of numerical probability would lead us to expect, we cannot doubt the effect of such conditions, especially when the three curves show the same effect in differing magnitudes.

To one who has lived in Colorado and has experienced these conditions, indicated by the lower end of the curve, the results, although perhaps surprising, are still fully creditable. The effect upon the school children, and indeed upon the people as a whole, is easily recognizable, and has been alluded to in the answers to the questionnaire coming from Colorado. The effect seems to be, primarily, an excessive stimulus to nervous discharge. The low humidity common to Colorado and the higher altitudes (see Fig. 12) makes this condition, to an extent, a permanent one, and has been alluded to in Section III. But even the chronic state of neurosis prevailing under normally low conditions of humidity is capable of intensification under those still lower, and these are what our curves give us. By 'the chronic state of neurosis' referred to, I do not mean a pathological condition, but a slightly increased nervous tension which all except the strongest and most phlegmatic feel. It shows itself frequently in mild insomnia or an occasional irritability of disposition, though not in melancholia. Even the horses are not free from the influence, and seem to be more nervous and excitable than their species in lower altitudes. This has been especially noticeable in highly bred horses which have been brought to Denver and Colorado Springs for track purposes. Not infrequently the effect has been such as to give their trainers much anxiety as to their ability to control them at all under the super-exciting conditions of the race. When, however, both with racing horses and human athletes, the effect has not been sufficient to bring on other complications, the available energy for rightly directed motor discharge has been increased, and some astonishing records are the result. The effect upon the mental worker is also recognizable. Work is, for the most part, turned off under higher pressure, with the necessary consequence that it generally cannot be so long maintained without a resulting condition of partial collapse ensuing, which demands a brief sojourn at a lower altitude for its relief. Ministers, teachers, lawyers and professional men generally feel this especially, and recognize the necessity of longer vacations than were needed by them when working at lower altitudes. The school year is shortened in accordance with this requirement, and even then the mental collapse of both pupil and teacher is usually greater than that felt by them at the conclusion of the longer school year in a more humid climate.

These effects are, I believe, not the results of dry air *per se*, but of its universally accompanying electrical condition. As was stated in Section III., the earth, for its entire surface, has been found to be charged with static electricity of a negative character. This is not supposed to be a property of the earth itself, but to be generated by the friction of objects on its surface and of air particles set in motion by the wind, under a discussion of which condition we shall again allude to it.

It is also supposed that the potential of this static charge at the surface of the earth is directly proportional to the non-conductivity of the surrounding atmosphere. If this were an absolute non-conductor, its effect would be similar to that of the glass of a Leyden jar, the earth representing one of the tinfoils of the jar and the clouds representing the other; but the properties of electrical conduction of the atmosphere decrease with its moisture or humidity, with the necessary consequence that those portions of the earth's surface which are insulated by a very dry atmosphere are continually being charged without being able to discharge convectively their burden to the clouds, or the higher strata of air which contain electricity of the opposite character. As a consequence, those regions where the humidity is low are normally more highly charged electrically than regions where the conditions of humidity are the opposite, and those regions themselves vary with respect to the potential of their charge as the humidity varies, the potential being strongest where the humidity is lowest. Actual experiment has proved that this potential is sometimes very high. Professor Arthur Schuster, F.R.S.,[1] says of it: "The strength of this electric field is not at all insignificant. If we wish to produce it artificially, between two parallel plates kept one foot apart, we should have to apply an electro-motive force sufficient, and sometimes more than sufficient, to light up the incandescent lights of our dwellings. The electric force is comparatively weak in our country (England), but 50 volts per square foot are constantly observed, and 100 volts are not uncommon; but *in drier climates the amount of force may be considerably in excess of these figures.*" The latter condition is certainly experienced in Colorado, though, so far as I know, no definite experiments have been made there to determine its magnitude. Although the electrical condition is observed for the surface of the earth, objects upon its surface would have a still higher charge. The increased tension for the static charge for projections or points upon the surface of the conductor is well known, and a human being or any other object upon the surface of the earth under such electrical conditions would show more than the full potential of its support.

[1] Smithsonian Report, 1895.

This tendency of the static charge to rush to points on the surface and thus discharge itself by convection gives rise to the phenomena of St. Elmo's Fire, or the so-called Castor and Pollux of the ancients, and also curious electrical manifestations sometimes observed on the summits of high mountains. It has been observed that the electrical potential of the air is less in the vicinity of larger towns and cities than in the open country, especially if it be free from woods. This is due in part to the effect of the numerous points presented by the buildings of the city, but still more largely, in all probability, to the discharging effects of the gases and columns of heated air arising from fires. Professor Schultze, in the paper already referred to, mentions the effects of these results of combustion, and concludes that their influence in reducing the potential in thickly settled regions must be tremendous. He says: "It follows that every fire burnt on the surface of the earth, and every chimney through which products of combustion pass, act like very effective lightning conductors, and would consequently discharge, slowly but surely, any electrification of the surface of the earth. The peculiar immunity of factory chimneys against damage by lightning appears from statistics collected by Hellman in Schleswich-Holstein, for which 6.3 churches per thousand were struck, and 8.5 windmills, the number per thousand for factory chimneys was 0.3 per thousand. It will be noted that although the action of lightning is mentioned in this quotation, the electrical conditions of the atmosphere which we have been considering are those normally prevailing without lightning or any visible electric phenomena. During a thunderstorm the potential of the earth's surface runs up to many times that of other occasions, but in the present study the mental effect of such conditions will not be considered. They are not frequent enough in most localities to form a factor in the cosmical environment; and, moreover, during the occurrence of an electrical storm the whole emotional condition of the school is so affected by fear or awe in the presence of such tremendous phenomena that no demerits could, with justice, be awarded which could be used as data for an inductive study like this.

In regard to the exact manner in which the electrical condi tion affects us little is known. Further study along the lines of electro-physiology and psychology may demonstrate this to a certainty; but for the present we must satisfy ourselves with what little light we may show upon the magnitude of this effect alone.

Wind.—The effects of the wind upon the emotional states of the various classes of individuals as disclosed by this study have been something of a surprise. In spite of the fact that we so frequently hear people deploring conditions of considerable movement, and asserting that the wind 'makes them nervous,' the curves taken as a whole fail to show that high winds for the climate of New York have any effect disastrous to mental quietude. In fact, these effects seem to be the reverse, for, in spite of many fluctuations—increasing as the data for the groups become less—the general tendency of the curves is downward as they show increasing velocities from the 100–150 mile group. This is not so plainly marked for the curve under consideration (Fig. 13) as in some of the others for adults, yet that portion for movements above 500 miles indicates noticeably less data than that for the 350 miles preceding. The marked deficiency for the lowest movements recorded I am at a loss to account for. It cannot be discovered by studying the meteorological tables that such conditions are the usual accompaniments of other meteorological states which have shown a deficiency of data; nor has it seemed to be a fact of general experience that days which were virtually without wind—as those registering less than 100 miles—have had a soothing influence. Yet the uniformity of the curves in this respect leaves no room for doubt. It may be that we have a solution in the relation which seems to exist between conditions of calm and the vitality of human beings. We alluded in our discussion of attendance in the schools to discoveries with regard to these effects upon the death rate in London, and we have here evidence of an increase for days when the movement was very small. If, then, perfectly calm days have a devitalizing effect, in accordance with our theories already stated, that a certain excess of vitality is necessary to conduct warranting demerits, we might expect just the deficiency indicated by the curve.

From the 150–200 groups the curve shows a gradual though slight increase in data till we reach the 500 mark, when it declines again, the excess for the highest movement being quite likely due to accident liable to the small number of data of the group. Any attempt to account for these peculiarities in the general tendency of the curve would be little more than guesswork, although several of the other curves show practically the same conditions. It would not seem improbable for their showing that while very slight and very excessive movements affect the vital forces at command in a negative manner, the velocities between these extremes have an opposite effect, though not a very marked one, and for the latter condition alone do we have excess of the occurrences studied.

Wind: DENVER.—A glance at the curve indicating the effect of the wind upon all the classes of data studied for the city of Denver is sufficient to show that its effect in such a climate is tremendous compared with what it is for New York. While those for the latter city do not diverge far from that of *expectancy* in the dry Colorado climate, movements of only a moderate absolute velocity are accompanied by nearly five times (470%) the normal number of misdemeanors in the public schools.

A fact most surprising to one who has lived in that climate is disclosed by the chart of monthly means (Fig. 8). It is that the movement of the wind for the year is only about one-half what it is for New York. This seems hardly credible to a Coloradian, for the severest criticism of his climate which he hears—indeed his own before he became so enthusiastic over its virtues as to fail to see its defects—was on this very score of wind. The curves under consideration, however, undoubtedly prove that he has been judging the velocity of the 'Colorado zephyrs' by the effect which they produce upon his mental state rather than by the anerometer record, and in this way has read into it a velocity which did not exist.

By noting the wind groups covered by the Denver curves it will be seen that the greatest movements occurring for all the year studied were between 450 and 500 miles, and though there was but a small fraction of our per cent. of such days, yet their effects, and those, in fact, of all movements above 250 miles, were tremendous. The undoubted cause of this was alluded to in our discussion of the effects of low humidities in such a climate. That the wind alone is not productive of such results is shown by the study of its influence for the climate of New York. There, those velocities which are so disastrous for Denver seem productive of no mental effects whatever.

As was said in the paragraph just alluded to, the electrical potential at the earth's surface nominally increases in an inverse ratio to the humidity. It may also be said with just as much truth that, humidity remaining the same, the electrical potential increases directly as the movement of the wind. The electrical state is in every respect one of static or frictional electricity, and, just as in the physical laboratory, the electrophorus takes less rubbing or the plate machine fewer turns to become charged on a dry day than on a moist one, so in the dry climate of the west a moderate movement of the wind produces effects which no hurricanes can bring about in the relatively humid east.

No very carefully tabulated data are required to demonstrate those effects in a qualitative way. When both conditions for producing the highest potential—that is, low humidity and high wind—come together the effect upon the pupils in the schools is very noticeable. It is no uncommon thing to hear a teacher deploring the conditions because of the fact that she could 'do nothing with the children.' In looking over the monthly observation sheets sent by a score or more of voluntary observers in the State of Colorado to the office of the Weather Bureau at Denver, I was struck by the force of some of the comments which were frequently appended. One, made by a physician in one of the smaller mountain towns upon the point under discussion, was: "Humidity for the month very low, which has set up an electrical condition that has set every one to fussing and fighting." Although he has considered the conditions upon which he has so tersely commented as due entirely to the humidity, the wind is also a factor in their production, and his characterization of the effects is too good to be omitted.

Character of the Day.—Surprising as it may seem to the many who have expressed their opinions in answers to the questionnaire sent out, on cloudy days the deportment in the school-room was better than on those of any other character, if we may judge by the number of demerits given. We might be inclined to be sceptical as to the validity of the curve, even though it be based upon the tabulation of 100,000 or more actual observations, and still prefer conclusions from our experience, did not all the other curves (banks excepted) show a similar deficiency of the conditions studied for cloudy days. Whole articles have been written based upon the supposed observation that the devil openly stalks abroad on gloomy days, and that crimes are multiplied under such conditions.

Dickens could hardly find sufficient excuse for a suicide or a tragedy of any sort except under the gloomiest condition of weather.

For the curve under consideration, partly cloudy days have quite a marked excess (9.8%); fair, about the *expected* number, and cloudy, the deficiency referred to.

On the vital theory of deportment which we have made use of in discussing some of the other results we should account for this deficiency upon the relaxing or devitalizing influence of gloomy weather; that under such conditions the children as a class lacked the energy to become disorderly and were comparatively quiescent, even though they were not intellectually energetic enough to do their best work. If we had a special marking system for inattention, or if I had been successful in obtaining the much desired daily marks for the class work, I am inclined to think that this hypothesis could have been proved. As it is, the work remains to be done. Marks for deportment would not be likely to show such conditions, except in a negative way, and this, I think, we have here.

The excess of demerits of partly cloudy days may be accounted for by taking into consideration the effect of different conditions of gloominess upon the emotions and the states of vitality. Cloudiness seems to indicate the negative character of the former, and decrease the available quantity of the latter. On fair days, then, although vitality would be at an excess, positive emotional states—or good nature—would so direct its motor output as to make conflict with the authority of the teacher not very liable. On partly cloudy days the available energy has only been lessened to a small extent; but a markedly negative emotional condition has been brought about which directs that considerable quantum of available energy into motor channels that are disastrous to good conduct. On cloudy days, although the emotional state would bode trouble, energy is lacking, and less active disorder is possible than when a moderate amount of both its elements is present. It may perhaps be argued that such an analysis is entirely unjustifiable; that the motives which influence the conduct of the child cannot be reduced to the twofold conflict of emotional quality and vital energy. Such a criticism may be entirely just, but will have weight in proportion to the validity of other hypotheses that may be brought forward to account for the conditions demonstrated by the data.

Character of the Day: DENVER.—This curve so nearly coincides with the one just discussed for New York as to leave little to be said. The quieting effect of cloudy days for the Denver climate is much more decided, and is somewhat surprising to one who has experienced it. Upon the basis of the compound influence of emotional states and vitality we must judge from this showing that for Denver the effect of cloudy weather was either less potent in negativing the emotional state or more potent in depleting the vital reserve, or both. But both of these suppositions are contrary to conclusions which experience of the occasional cloudy days of the Denver climate would lead us to make. We seem to be conscious at such times of an especially depressing effect upon the spirits, though what the influence upon vitality may be is hard to state from introspection. The Denver curves for Suicide and Murder show an opposite effect for cloudy days from that indicated by this curve, and a fuller discussion of the probable causes will be given in the sections which treat of those classes of data.

Precipitation.—The relative number of demerits given upon rainy days and upon those in which there was no precipitation is what would be naturally expected from the study for the character of the day—that is, an excess for dry days and a corresponding deficiency for rainy days. Since fair days are usually of the former character, and those on which there is precipitation of the latter, what has been said of them is valid for our present topic.

SECTION V.

SUMMARY OF OTHER CLASSES OF DATA.

It is my purpose in this section to summarize as briefly as possible the effects of the definite meteorological conditions studied upon the other classes of data mentioned in Section II. It must take the place in this paper of more exhaustive discussions of the separate classes.

OCCURRENCE.—A glance at Fig. 9 shows that for the peculiarities of conduct there is a somewhat marked increase in occurrence for the hot months. In some cases (Male Assaults notably) the curves resemble very closely those of monthly means of temperature (Fig. 8). For the Insane and Discipline in the Penitentiary the increase is much less marked, yet plainly noticeable. The curve for Suicide shows the increase much earlier in the year: so early in fact as to make it seem improbable that this is due to heat alone. Morselli ('Suicide,' International Science Series), in a most exhaustive study of suicide for European countries, has called attention to a peculiar fluctuation in the occurrence curve which coincides almost exactly with the results of this study. For many of the countries considered by him he found the maximum number for the year occur either in May or in July, with always a large excess for the spring months. That this season of the year, when all nature is in its sunniest mood and smiling in its brightest garb, should be chosen by so many as the time for exit from a world so inviting, is contrary to what an analysis of the problem would lead us to expect. There are, however, certain facts discoverable which tend to account for such seeming anomalies in conduct.

First.—It is recognized that at the end of the winter months the surplus of vital energy is at a very low point; this is not shown by the death curve, for at this period of the year diseases that usually prove fatal are not prevalent (those of the lungs and intestinal organs); but the strong man feels it, and people generally, as is shown by the demand for the so-called 'spring medicines.'

Second.—Under conditions of depleted vitality the general emotional tone is lowered, and sometimes brought to a negative extreme.

Third.—Under such conditions of vitality impulses to action would be less liable to be inhibited; and consequently more apt to result in a motor reaction. All these conditions would tend to increase the number of suicides; but I am inclined to think that there is another cause which, added to those already mentioned, is a more immediate one. It is the conscious or unconscious contrast of the recognized low condition of vitality with the exuberance of energy and life in the rejuvenated nature about, making one who is weak feel that the struggle against the resistance to life and progress, in competition with a world so virile, is hopeless. Beside this, there is the remembrance of other seasons when the increased animation of the world about throbbed in his own pulses, bringing greater hope and aspirations. All nature is again in the flush of youth, as on so many happier occasions, when the mood of the subject was in full harmony with her; but in his present enfeebled state the vitality and life of the newly awakened spring make the contrast such a terrible one, that he is apt to cry with the author of 'Locksley Hall:'

> "Comfort? comfort scorn'd of devils! this is truth the poet sings,
> That a sorrow's crown of sorrow is remembering happier things."[1]

And to put an end to a hateful and unbearable existence.

This argument is, of course, not intended to account for suicide, but for the *excess* of suicides shown for the spring months. It would not in any sense account for self-destruction through cowardice or an inability to face the consequences of some definite misdeed, but only such as resulted from a chronic state of low vitality, either physical or mental.

The occurrence curve varying most widely from the normal is that of Discipline in the Penitentiary. In explanation of the fact that for the heated period there is no increase in the number of persons under special restraint I need only mention the fact that during the summer there are many more at work out of doors, and by the system of rotation a large number of the convicts are thus allowed to work off their surplus energy. The fact, too, that misconduct debars one the privilege of enjoying the outer air undoubtedly proves an incentive to good behavior. To account for the fact shown by the curves for death and for suicide, that those occurrences were much less frequent for August than for July, I should say that for the two years for which they were studied (1886–87) July was much the hotter month. The curves would probably not be normal for any considerable period of time.

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[1] See Dante's Divine Comedy, Canto V.:

"No greater grief than to remember days
Of joy when misery is at hand."

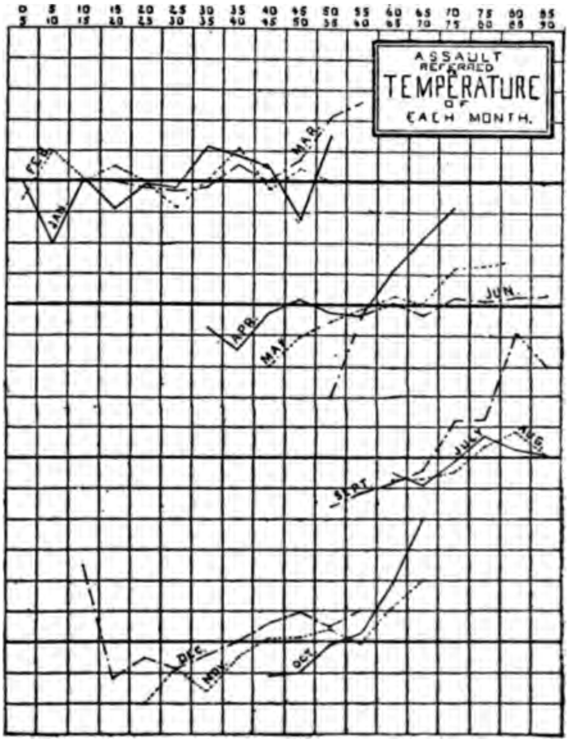
TEMPERATURE.—

> "For now these hot days is the mad blood stirring."

This quotation may, perhaps, be taken as an epitome of the results shown by Fig. 10. From them, we see that there are no exceptions to the fact that excessive heat is accompanied by an increase in occurrence. Generally speaking, this increase is somewhat gradual from the lowest temperatures to a point varying for the different curves, but uniformly somewhere between 65° and 80°, at which the increase is very much more rapid. For suicide alone is there a similar excess noted for very low temperatures; and this fact may, perhaps, be accounted for by the increased misery such conditions bring to those who are not properly housed. It will be noted that the more rapid increase for greater heat begins at a lower temperature for this class of data and also for assault. This fact leads to a more definite study of temperature conditions as follows: Since the more rapid increase for assault began at 10° or 15° lower than for most other classes, it seemed evident that there might be conditions of heat which would fail to show upon these general curves as effective, even though their influence be considerable. Such would be unseasonable heat—that is, excessively hot days *for the time of year*, though in actual temperature only equal to comparatively cool ones for a hotter season. To explain by using a concrete example:

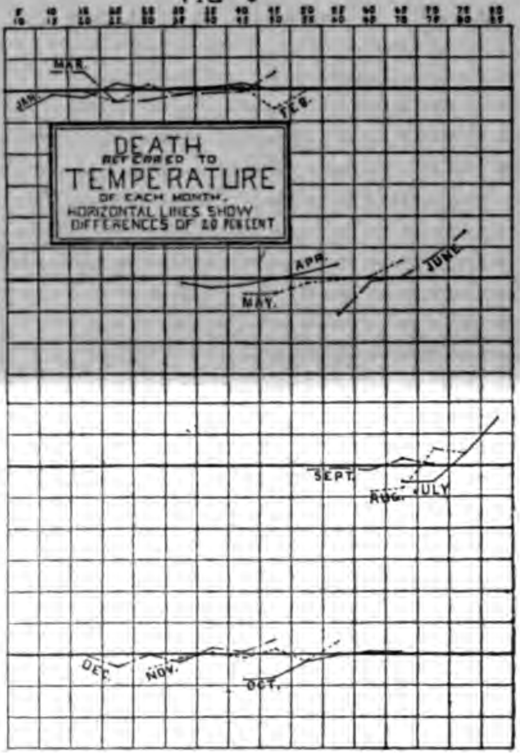
An April day of a mean temperature of 70° might, for all this curve shows, have a disastrous effect upon general conduct; yet in the study of that condition for the year the fact might be entirely concealed by the reverse effect of an August day of the same temperature. In order to discover such mutual negativing of results Fig. 5 was prepared, upon which are shown the temperature curves for male assaults for each month of the

FIG. 5.



year. Although this figure is not elsewhere explained, it is constructed in every respect upon the same principles as the others, and shows some exceedingly interesting things, especially when studied in connection with Fig. 6, the only other figure showing monthly temperature curves. The curves interpreted show that in Jan. and Feb. the temperature variations have very little effect—that is, the curve has no very marked general tendency either up or down, the fluctuations being probably due to accidental conditions which a larger number of data would tend to eliminate. In March the hot days are beginning to have their effect, which in April is at its maximum, decreasing as the heat of summer comes on, and increasing again, till an autumnal climax is reached in October, with another decrescendo effect for hot days as winter approaches. The unusual and interesting fact demonstrated here with a certainty that cannot be doubted, is that the unseasonably hot days of spring and autumn are the pugnacious ones, even though the actual heat be much less than that for summer. In fact, it will be noticed that for the excessively hot days, registering a mean of from 80° to 90°, the number of assaults for at least three of the months upon which that condition is reached (July, Aug. and Sept.) decreases. We might infer from this that conditions of heat up to a certain limit are vitalizing in their tendency, while at the same time irritating, especially when we are not dressed in accordance with the demands of comfort; but above that

FIG 6



limit heat is so devitalizing in its effect as to leave hardly energy enough to carry on a fight. This devitalizing influence of high temperature is especially shown in our treatment of the death rate, indicating, as it does, that deaths increase with an absolute increase in temperature, and not with unseasonable heat, as was the case for assault. The special curves (Fig. 6) for the first three and the last two months of the year show little increase as affected by temperature. For the intervening months we have a somewhat gradual rise with the temperature, which reaches its climax in the months of June and July. That August does not show as marked an effect is undoubtedly due to the lower mean for the month, alluded to under our discussion of occurrence in this section. The results of these curves are more interesting in the setting which they furnish for the similar one for assault, than for any absolute value they have of their own. The facts which they disclose are only those which would be naturally expected—that is, that under conditions of excessive heat the general metabolism of life processes is so increased as to leave little surplus of vitality to be drawn upon by the accidents of sickness or other contingencies.

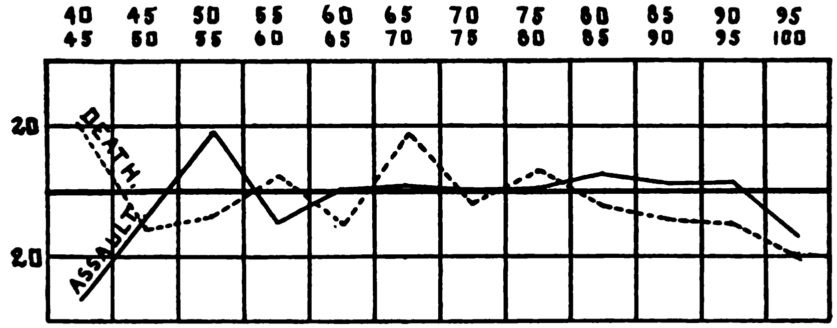
The very excessive increase in the number being disciplined in the penitentiary under conditions of great heat may, it seems to me, mean either one or both of two things: that those at work upon the grounds were emotionally affected by such conditions, while still retaining energy enough to be actively ugly—energy normally being at a surplus because of the light drafts made upon it in such institutions; or it may be an evidence of the poor ventilation and sanitary condition of the cells or rooms of detention. The school children—the only other class whose deportment we have studied for indoor condition—show reverse effects for temperatures as registered at the Weather Bureau. In accounting for this, I argued that the cool, well-ventilated school-rooms were undoubtedly a factor; for the conditions as found at the penitentiary we might argue the reverse. The relations of the curves for the different sexes where they were studied separately (as in the case of Assault and Insane) lead to an interesting conclusion: it is that the effect of heat upon females is greater than upon males. This is shown both in an increased pugnacity and in a greater mental unbalancing. Especially in the prevalence of assault is it noticeable. Starting at the lower temperature with a deficiency much greater than that for males, the curve indicates a somewhat gradual increase to an excess of 100%, or double the expected number, for the temperature groups 80° to 85°, at which point it makes a drop to 33%. The curve for males shows neither extreme so far from expectancy, nor is the drop at the end so marked. This sudden falling off of assaults for the most excessive heat is, I believe, a very interesting fact; but since conclusions are drawn from it which are discussed in the next section, I merely call attention to it here. The greater sensitiveness of woman to weather conditions was noted by at least one of the teachers in discussing the answers to the questionnaire. The physiological conditions which bring about an unequal distribution, in point of time, of demand upon the vital energy upon the sex are undoubtedly its causes. Without question, during periods when this demand is greatest the emotions are less stable and the weather effects much greater than for periods when those demands are less.

BAROMETER.—There is a greater resemblance between the effects upon the different classes of data studied, of those meteorological conditions of which the barometer is the measure, than of those of any other weather state (see Fig. 11). With the exception of the curve for Errors in Banks, the various curves might be superposed, and, except for minor fluctuations, would practically coincide. Death and Suicide were not, however, studied for this condition, owing to a difficulty in getting the record for the years 1886–7, for it is possible we might find a somewhat greater divergence. The fact disclosed is that—save for the exception cited—there is an excess for all occurrences for relatively low readings, and a corresponding deficiency for high ones. As has been stated elsewhere (Sec. III.), it does not seem probable that the definite state of the atmosphere revealed by the barometer is itself the direct cause of the variation, so much as other weather states which are their concomitants. Since, however, the other curves so nearly resemble that for deportment in the schools, which was somewhat fully discussed in the chapter just referred to, we need mention here only the divergent one of Errors in Banks. As a glance at it shows, there seems to be no general effect of the different barometric conditions upon their occurrence, although errors seem to be slightly less in number for conditions of low barometer than for high. As a basis for its consideration, we must conclude that the other curves indicate a less full control of the emotions for low barometers than for high. Reasoning from these facts, my conclusion is that the errors studied are more in number when mental—or at least emotional—states are under most perfect control. This, however, seems contrary to all opinions based upon introspection or general observation, and leads to an analysis of the conditions under which the errors were made in search for any other hypothesis in explanation of the facts. A possible one is found as a result of conference with the offending individuals themselves. It is that on some days they seem to feel a confidence in themselves which leads them to pass off work as perfect at the first attempt; a computation is rapidly made, or a column of figures added, and another piece of work undertaken, without a second thought as to the correctness of either. This is the case on days when they feel their *best*. On other days a greater possibility of error seems to be recognized, based upon the way they feel, and greater care is taken with each process, or the work is even done again in order to prevent errors which are thought to be probable in their then mental state. If these facts be true, the result would be just what we find for our curve. An increased tendency inhibited with more than a compensating care would give a negative result; yet in this case our results are not those of weather effects upon mental states, except as those effects are especially compensated for by volition, and we cannot quote the readings of this curve as normal for the influence of the barometer upon intellectual activities. It is quite probable that, at least for the teachers in the schools, there is a similar restraint based upon recognized tendencies, which would serve to negative the tabulated effects of the weather upon conduct. In fact, if this study is to be of any practical value to them, it can be so only in pointing out the conditions under which such restraint must be exercised, as well as allowances made for others.

HUMIDITY.—The curves for humidity (see Fig. 12) may be divided into two general classes: those showing a decrease of occurrences as the humidity increases, and those which show no marked tendency either way. To the former class belong those of Assault, the Insane and Discipline in the Penitentiary; to the latter, Death, Suicide and Errors in Banks. No class of data studied shows an increase, unless it be a slight one for the last named, with increasing humidity. This fact is, I believe, rather surprising, for it seems to be a prevalent opinion that occurrences of the nature considered are excessive upon humid days. Indeed, it is so certain that under such conditions we, in some indefinable way, feel ourselves out of our normal balance, that I should be inclined to doubt the correctness of a single curve; but with six (including that for the schools), based upon the results of nearly 100,000 data, all showing the same trend, we can hardly doubt their validity.

It will be noted also that all five of the curves treated in this section showing the general decline with increasing humidity make a somewhat sudden drop for the highest groups. Nor can this drop be accidental, for reference to the curve of Normal Prevalence shows that too large a percentage of the data (3%) occurs in the 95–100 groups to make this probable. It must, seemingly, be due to weather effects. In the light of facts demonstrated in other parts of this paper, it would seem as if the condition of humidity brought about the results indicated, through its effect upon the reserve energy. An excess of energy is required to produce the abnormalities in conduct treated in the first class—that is, Assault, Misdemeanor in the Penitentiary and, perhaps, the active symptoms of Insanity; while Death, Suicide and mental inexactness are results of the deficient state. Yet both the chilly, damp days of winter and the muggy, sticky days of summer have high humidities. This fact led me to question if an emotionally quieting effect might not accompany the former, and one of a reverse influence, the latter; yet the two so negative each other's effects when taken together as to fail to represent fairly the condition. Fig. 7 was constructed as the result of attempts to show the effect of humidity when an accompaniment of high temperatures only. It was made for male Assault and Death as the two classes having the largest number of data, and consequently less liable to be affected by accident. In their preparation the effect of humidity upon their occurrence for days when the temperature was 70° or over was studied; in other words, an attempt was made to find whether humid, hot days, or dry, hot days were more productive of these results. We see that even for those days whose temperature was within an arbitrary group of 70° to 95° the varying condition of humidity had but a comparatively unimportant effect, and that of a character which we should hardly expect. If the curves show any general tendency, it is one of decrease of occurrence for high humidities, for both curves show a somewhat marked deficiency for the highest group.

FIG. 7.



However, I do not consider these curves of much value, for two reasons: First, the number of days upon which they are based is too small. There is an average of only 61 days each year having a mean temperature above 70°, and this gave us but 122 for the Death curve and only 2 each for the humidity groups 95–100, 40–45 and 45–50, and but 8 for those of 85–90 and 90–95. This number is altogether inadequate, as accidents might easily affect the results. Secondly, the excessively hot days of our extended temperature groups were seldom those of the highest humidity. When those two conditions do come together the results are known to be very disastrous; but for the period studied the reverse was true. This fact could not help affecting our results very materially. Our temperature curves (Fig.

10) have shown the great effect of high temperature, and this means that on the hottest days would be expected the greatest number of Assaults and Death. If, however, those hottest days were, as seems to have been the case, almost universally days of low humidity, we might have the result shown by Fig. 7, or even a more marked excess for lower humidities. This source of error might have been lessened by making use of a smaller temperature group; but this would have materially cut down the number of data, and so made prominent the other horn of our dilemma.

To return again to the general curves upon Fig. 12. The slight excess for lower humidities may, perhaps, be ascribed to the increased electrical potential for such conditions. Although this electrical state has been described (Section III.) as existing at every point upon the earth's surface and for any climate, it has not been found to be very great except for lower humidities than are common for New York (see Denver curve), at which point it makes a great jump. The fact has not been fully determined, although it is quite probable that the potential bears a somewhat constant inverse ratio to the humidity. If this be true, our excess of data shown for low humidities may be accounted for as was the very marked one for the Denver school children under still lower conditions.

The curve showing the prevalence of Errors in Banks is interesting, because it comprises the only class of data which are based upon intellectual processes pure and simple. The excess for the lowest humidities might indicate, if it be not due to accident, that the intellectual balance is more disturbed by the increased electrical potential than is the emotional. The gradual rise in the curve from the 50–55 to the 95–100 group indicates that the devitalizing effect of increasing conditions of moisture have so exhausted the energy applicable to mental processes as to affect their regularity and perfection.

WIND.—Generalizing from the effects of the wind upon the occurrence of data as shown by the curves upon Fig. 13, I should say that calm and high winds are accompanied by a decrease in their number, while moderately high winds show a corresponding increase. The exceptions to this are death, suicide and errors in banks, for which the general maximum of occurrence is during calms. These disclosures are, I believe, somewhat startling, as our own feelings in the matter would lead us to think otherwise. Daily movements of from 100 to 200 miles, for which conditions many of the curves show the maximum of occurrence, are those ordinarily considered most agreeable and bracing. That the greatest number of Suicides, attacks of insanity and assault should occur then does not seem easy to understand, especially in the light of opinion expressed in answer to the questionnaire, for such days could hardly have been classed as 'Windy.' There is, however, the possibility that on such days nearly all the movement of the wind registered for the day took place within a limited time, in which case they might naturally have been considered so.

Perhaps the effect of conditions of calm upon different individuals varies with the personal temperament and with the time of year. It seems certain that a calm day during the heated summer season is depressing, while a similar condition for other times of the year has a reverse effect, both emotionally and in its relation to vital energy. The deficiency of occurrences as shown by the curves for such conditions may be contributed to by both these states: the former, by so depleting the energy at command as to lessen the numbers; and the latter, by increasing the energy without affecting the emotions in such a way as to direct its output into channels productive of misdemeanor. In this case intellectual activities might, if we had sufficient data, be shown to be the result.

It will be noted that death and suicide are both somewhat excessive for conditions of calm. In regard to the former, the fact was noted in a reference to studies made in London. In a large city like New York the fact may be due to the lessened proportion of oxygen in the atmosphere, because of the depletion by respiration and fires without fresh air being brought in to replace it, and the consequent increase of carbonic acid gas. Dr. J. B. Cohen[1] has shown that the proportion of this gas in the atmosphere in the center of the town of Manchester, England, averages nearly three times (for some observations more than four times) that in the outskirts. Although he makes no reference to the fact, it would seem probable that the differences which he found may have been due very largely to differences in circulation brought about by varying velocities of the wind. Certainly when the movement was very violent but little difference could exist in the composition of the city air and in that of the country. Recognizing the importance of oxygen and the disastrous effects of carbon dioxide upon the metabolism of life, it would not be strange if the conditions shown by our curves were in some way influenced by their varying quantities. In fact, basing conclusions upon the theory of vitality already expressed, we should expect just what is shown by them—that is, if an insufficiency of oxygen and the universally accompanying excess of carbon dioxide be devitalizing conditions, under them we might expect a deficiency of misdemeanors due to activity and an excess of those of a passive nature.

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[1] See Smithsonian Reports, 1895, p. 573.

Those of the first class mentioned would be assaults, misdemeanors in school and penitentiary and initial attacks of insanity, all of which show a deficiency for calms. To the latter class would belong death, and possibly suicides, both of which are excessive for the same conditions. Errors in Banks might also, it seems to me, be classed with the latter. Certainly deficient energy, unless in some way compensated for, would tend to introduce imperfection into intellectual processes. For this class of data we have the *expected* number for calms.

CHARACTER OF THE DAY AND PRECIPITATION.—The effects of days of different characters, according to the nomenclature of the Weather Bureau (as shown by the curves upon Fig. 14), seem also contrary to general opinion upon the matter. With the sole exception of Errors in Banks and Death, there were more occurrences of every class studied for fair days than for cloudy days. In some cases, however, there were more for those characterized as partly cloudy than for fair. The curves for Precipitation, with the exceptions noted, also show universal excesses for those days on which there was no rain or snow. Considering the greater excuse one has to be out of sorts on rainy or cloudy days, as when the pleasure of a picnic or excursion into the country has to be foregone because of unpropitious weather, this fact is quite surprising. Even the suicide chooses the fair day for self-annihilation (except in the Denver climate), and seems to have a very marked aversion to rainy weather. The relation between the curve for Character of the Day and that for Precipitation for the same class of data throws additional light upon the exact kind of day chosen. Attention has been called to the fact that not all cloudy days are rainy days; nor are all those upon which there is precipitation necessarily characterized as cloudy, or even partly cloudy. Comparing the relative deficiencies of a given class of data for cloudy days and for those showing precipitation, we can discuss the effects of those gloomy days which we all know so well, when the sky is overcast or perhaps the atmosphere full of fog, but on which no rain falls. As an example, we may compare the curves for suicide under the two conditions, and we see that for cloudy days there is shown a deficiency of 4%, while rainy days indicate one of 14%. This means that although the number for both conditions was less than *expectancy*, there were relatively more suicides for cloudy than for rainy days. Now it would be safe to say that nearly all the rainy days were characterized as cloudy, but that the former only made up a portion of the latter. The remainder, which would be cloudy, but not rainy, would have to account for the difference of 10% shown by the two curves. This must mean that, of the cloudy days, those which were without rain were accompanied by the greater number of suicides.

All the sets of curves may be considered in the same way, and, as a matter of fact, nearly all would show the same effect for days overcast yet without rain. I recognize a possible fallacy in this argument, from the fact that in each of the curves for character of the day three conditions are shown, while for precipitation there are but two; yet for many of the classes of data the number for at least one of the three characterizations of the day is so near the *expectancy* that it can hardly have much effect upon the result.

The question might be asked as to the effect of foggy days. No special study has been made of them, but an analysis of the curves under consideration might in part answer it. Days upon which there is fog might come under any one of the five conditions of the two curves. If they were accompanied by no precipitation, they would be characterized as fair, partly cloudy or cloudy, according to the duration of the fog. Most of the foggy days in New York, however, are accompanied by more or less precipitation, and I am inclined to think that those portions of the two curves under 'Cloudy' and '+ 01 inch' would include a great majority of them. It would seem that such were accompanied by deficiencies in occurrence in all the data studied except Errors in Banks, and perhaps Death, which contradicts itself in this respect slightly.

Upon the same theory which has been stated in other paragraphs, we should say that such days are devitalizing. There are reasons for believing that they are so from a mere study of the chemistry of the atmosphere at such times. In the paper referred to under the topic 'Wind,' a further analysis of the air is given for clear and for foggy days. The figures there show that under the latter condition the volume of carbon dioxide is from two to three times what it is under clear skies, the volume increasing in proportion to the duration of the fog. We found that when a similar excess of this poison existed in the atmosphere because of lack of wind to carry it away all the curves bore the same relation to the *expectancy*, and each is corroborative of the other's evidence that devitalizing atmospheric conditions, whatever may be their effect upon the emotions, are inhibitive of action.

SECTION VI.

CONCLUSION.

In conclusion I wish to submit the following theses, and outline in brief arguments in their defence based upon the results of this study:

First.—Varying meteorological conditions affect directly the metabolism of life.

Second.—The 'reserve energy' capable of being utilized for intellectual processes and activities other than those of the vital organs is influenced to a marked degree by meteorological conditions.

Third.—The quality of the emotional state is plainly influenced.

Fourth.—The reserve energy and the emotional state are both factors in the determination of conduct.

Fifth.—Conduct, in the commonly accepted sense of the term, Death and Intellectual and Physical Labor bear very different relations to reserve energy.

The first thesis must be supported entirely from our study of death rate and disease, as in all the other classes of data we deal with a utilization of the 'reserve energy.' Any alteration in the death rate traceable to the weather is directly in support of this thesis, for disease is but another name for some pathological condition of the body, and any pathological condition is but a derangement of its metabolic processes. Even if this derangement be due immediately to the action of some germ, any variation in the activity of the processes which are shown to be closely related to differing meteorological conditions must be attributed either to the effects of these conditions upon the germ or the organism supporting it. Since germs have been proved to be such hardy little creatures, we are forced to suppose the effects to be upon the organism. For whatever meteorological conditions, then, we find a well-marked excess or deficiency in the death rate, or even the attendance at the public schools—if our supposition was correct that this was influenced by health—we may conclude that an effect upon the metabolic processes of life is indicated. Such an effect may be either favorable or unfavorable to health. Favorable conditions would be shown by a decrease in deaths and an increase in school attendance; unfavorable conditions, by the reverse. A study of the curves for Death and Attendance shows that generally low conditions of temperature, humidity and wind are favorable, as shown by a deficiency of deaths and an increased school attendance, while partly cloudy and dry days seem to be somewhat so, if we make allowance for the direct effect of such conditions upon the ability of the school children to get out. On the contrary, high temperature and humidity, and moderately high winds, together with rainy, fair and cloudy days, are somewhat unfavorable. These conditions seem so different, basing our judgment upon general experiences, as to lead to a further analysis of the unfavorable conditions. Some of them have been shown to be of such a character as to accelerate the oxidizing processes of life, and others to retard them: the former I shall call *anabolic*; the latter, *katabolic* conditions. High temperature, high winds (better ventilation), fair days (see page 72) with low humidities as an accompaniment are *anabolic;* while low temperatures, high barometric conditions, calms, rainy and cloudy days and high humidities, because of their opposite characteristics, are *katabolic*. Now it may be seen that health, which means a metabolism of the definite rapidity demanded by nature, and brought about by a long series of natural selections, may be affected disastrously either by an acceleration or a retardation of those processes. A fire may go out either from lack of fuel (too rapid oxidation) or from lack of draft (too little oxygen). The same fact is shown in a definite manner by our study for the metabolic processes of life. Those conditions which I have termed *anabolic* produce too rapid a metabolism to be borne by the weakened organism, while the *katabolic* so lessen the production of available energy as to reduce its quantity below the minimum required for life or health. A study of the various curves of the death rates and school attendance shows that, generally speaking, the so-called *katabolic* conditions are the vitally depleting ones. The marked exception is found in the showing for high temperature (mentioned later in this section); but the fact is plainly true for calms, high humidities, low barometric conditions, and to an extent for cloudy and rainy days. These conditions have all been classified as *katabolic*. For them the death rate is high and attendance low. This is not strictly true for the death rate upon rainy days when compared with *expectancy*, but taking the curves of conduct as our basis of comparison rainy days showed a very marked excess. These relations, considered in the light of our already defined use of the term *katabolic* as applied to meteorological conditions, show that vitality is depleted and that the spark of life goes out because of the retardation of the metabolic processes, rather than an acceleration of them to an extent that depletes the cell structure. If an animal were placed in an atmosphere of pure oxygen, death would ensue from the latter cause; such a condition would be *anabolic* in the extreme. Although the variations in the life-giving element of our atmospheric air, or of carbon dioxide, the death-dealing component, are not great, still they are of sufficient magnitude to be effective, and we must conclude that the vital fires are quenched for want of the supporting medium, rather than that they burn out from lack of fuel.

My use of the terms *anabolic* and *katabolic* is open to criticism, and I should not attempt to uphold it except that there seem to be no other available terms, and with the exact definition which I have given of their use in this study, a definite conception may be imparted without circumlocution.

Second.—If the first thesis be granted, the second is valid as a direct consequence; the sum total of the vital energy of any organism would consist of the energy which is being used by and is necessary to, the vital processes of living, plus a remainder, which may be utilized for intellectual processes and the motor activities accidental to life. The latter I have designated as *reserve energy*. Granting the first thesis, that the metabolism of life is affected by the condition studied, it must follow that the sum total of energy which depends directly upon such processes must vary also. The sum total of energy is made up of the potential energy of vital reserve and the kinetic of vital processes; but this latter remains more nearly constant, so that any variation in the sum must be largely a variation in the other component—that is, reserve energy. This is an important fact, and on it depend largely the conclusions drawn from this study.

The reserve energy is interesting in its relation to the life history of an individual. During the period of growth it seems to be universally excessive for persons in good health. It shows itself in the play and spontaneous activities of childhood, and the athletic sports and intellectual development of youth. During the period of maturity it has a strong balance in its favor, unless depleted by special demands made upon it; while old age gradually lessens its quantity, till the sum total of bodily energy only equals that being used in life processes. Under such conditions any extra demand necessitates a draft upon the latter, and if it be too heavy, the whole process stops. But beside the life rhythm in the reserve energy, there are many others due to the accidental conditions of bodily health. We all recognize them, and they need not be discussed here. The rhythm which we hope to demonstrate is one of less marked fluctuation; one not of health and disease in the ordinarily accepted sense of the terms, but one of exhiliration and depression, of activity and lassitude, of good spirits and poor. Not these conditions as the traceable effects of actual bodily disease, but coming whence we cannot tell, and why, it is beyond our power to discover. This rhythm it is that the subtle agency of weather seems to affect. As a consequence of our method of reasoning, those definite meteorological conditions which were designated in the discussion of the first thesis as *anabolic*, would be those which in this varying weather rhythm would have the greatest reserve energy, the reason being that for them the metabolic processes which are the generators of bodily energy are accelerated more than the demands made upon them by the vital processes are increased. There seems to be one marked exception to this generation of energy and the demands made upon it by vital processes, and that is, for excessively high temperatures. For such conditions the vital demand exceeds the production, and the reserve is depleted or exhausted. This is shown by the great increase in Death and the sudden decrease in Assault shown upon Fig. 10 for the highest groups, but especially by the curves upon Fig. 5. The latter show no increase in Assault for the highest temperatures of July and August, while the actually cooler days of April and October, though the hottest for those months, show marked effects. It seems to me probable that the low vital reserve for excessively high temperatures may be due in part at least to the increased energy required by the accelerated secretion of the glands belonging to the excretory system. Large amounts of perspiration can be produced only at the expense of some form of energy, and since that is a process certainly abnormal at such times, it may be something of a drain upon vitality. This theory is, perhaps, corroborated by the etiology of sunstroke. This only occurs when perspiration has ceased, at which time the high bodily temperature and the activity of the metabolic process may mean more than mere failure to reduce the former by evaporation. The conditions which were characterized in the discussions of the first thesis as *anabolic* were high temperatures, considerable movements of the wind, fair, dry days and low humidities; but with few exceptions, and none for conduct in its accepted sense, all those conditions are accompanied by excesses in the various classes of data studied. Our conclusion then must be that increased vital reserve tends to increase the number of misdemeanors classified under Conduct.

Third.—This thesis must be defended by means of an analysis based solely upon introspection. Considering the importance that has been laid upon the question of reserve energy, it would not be safe to conclude that an excess shown by any one of the curves, even of such occurrences as Assault, is due to the effect of the conditions of the emotions alone. We are all, however, conscious of the emotionally depressing effects of some states of the weather. Nothing even that any inductive study might show could disabuse us of our belief. For ourselves, too, we are certain as to the exact conditions of weather which have a most negative effect upon them; these may vary with different individuals, but after all they are pretty constant. Just what the middle link of the chain which connects body and mind may be is unimportant from our present point of view. It may be, and perhaps is, something closely connected with the reserve energy; if so, we are not certain, and can disregard the 'quia' for the 'quod.'

It is safe to say that high conditions of temperature and humidity, cloudy and rainy days, and for many people high winds, are generally productive of more or less negative emotional states, while moderate and cool temperatures, low humidities, mild winds and clear, dry days are usually positive in their effects. With this, we leave the thesis for the next.

Fourth.—In the discussion of this thesis we must discover the relation existing between reserve energy and the emotions, for meteorological conditions showing marked excesses or deficiencies of data. A disorderly act of the nature of an assault or misdemeanor in the penitentiary, can only occur at such a time as there may be both energy and inclination, or impulse to undertake it. The former must bear some relation to the reserve energy; the latter, to the emotional state, which would at the instant be negative in its quality. We have traced the relation between the occurrences taken as data of deportment and the reserve energy; we have stated that—arrived at by introspection and general experience—the days upon which we most often find ourselves 'out of sorts' (a negative emotional state) are the hot, humid, cloudy and perhaps rainy ones, some of us reserving the right to be hard to get along with when the wind is high. Such are the days when, all other things being equal, we should be most liable to have trouble with teacher or fellow-man, *if the emotional state were the only factor*. But we find that for some at least of these 'ugly' days the numbers of misdemeanors alluded to are much below expectancy. Such is the case—mirabile dictu—for the humid, the very windy, the cloudy and the rainy days. We should be at a loss to account for such a showing did not the conclusions from our study of the reserve energy come to our aid; for all the conditions noted this was found to be very much depleted. This fact is undoubtedly the cause of the deficiencies shown. A most ardent desire to hurt somebody is not very dangerous if there is nothing to back it up. A most uncomfortable atmosphere might hover about, and a record of profanity might show some interesting things; but *inclination* alone will at least get no one into the police court. Reserve energy, on the other hand, seems a most dangerous thing to have about, as far as personal conflicts are concerned. With the consciousness of power to maintain one's position, even by strength of arms, the opportunity under the most favorable emotional condition seems likely to arise. At any rate, our curves show the balance in favor of the influence of reserve energy, accompanying the best of spirits, over vital deficiency and most unhappy states of mind. If we could have made this study in colonial days, and have taken our records from the ducking-stool, the curves might have been very different.

Fifth.—In the discussion of the first thesis we showed the relation existing between the Death rate and the various meteorological conditions studied. In considering the second and fourth we have shown that relation for the data of Conduct.

Reference to the statements there made or to the charts themselves shows that in some notable respects the curves differ from one another. I wish here to compare the curve for Errors in Banks—the only one showing intellectual processes pure and simple—and that for Suicide, with those for Death and Conduct, to see which one of the latter the former more nearly resemble. Incidentally, too, I shall wish to refer to the study of Physical Strength, as exerted to its maximum, in the tests which have been made under my direction in the Columbia University Gymnasium. The results of the latter study could not well be shown upon the charts published with this paper, and as no definite discussion of the problem is included, no special figures for the illustration of the few points here made are given.

First, the general effect of conditions which I have designated as *katabolic* has been to increase the death rate.

Second, the general effect of *anabolic* conditions has been to increase the data of Conduct. There are some exceptions to both these statements, notably that of high temperature upon the Death rate, which has been touched upon and hypothetically accounted for; still, as a whole, the statements are valid. Tracing the curve for Errors in Banks upon the various charts, we are convinced that they more nearly resemble those for Death than for Conduct. On the temperature chart, Fig. 10, all the curves (except that for the school children) show the same general tendencies. On that for humidity the curve for the Errors in Banks closely resembles that for Death, both showing excesses for high humidity in marked contrast to those for Conduct. For Wind, it resembles the death rate in showing no deficiency for calm, though for the rest of its course it is more similar to the Conduct curve. For cloudy days it and the Death curve are the only ones giving expectancy or more, and for rainy days they are the only ones not showing a marked deficiency. Without discussing this curve further, we assert that its resemblance to that for Death is much closer than to those for Conduct. This is, perhaps, only what should have been expected, since in both the intellectual work of the bank clerks and the vital processes of life actual demands upon the reserve energy are made.

The barometric conditions, which have been wholly omitted from the discussion in this chapter because they were not available in our study of the death rate, show an interesting discrepancy for the errors in banks curve. This cannot be interpreted in terms of comparison with anything but the curves for Conduct; still they are perhaps suggestive of the position of that for Death, if it were computed.

Without discussing the Suicide curve in detail, we can say that in many respects it is midway between those for Death and Conduct in many of its showings. For humidity in particular it closely resembles the former. An analysis of the mental and physical states which would most likely give rise to suicide would suggest those of a negative quality coupled with those of a depleted reserve energy. Each of these is the accompaniment preëminently of one of the classes of data which we have been considering; the former of excesses in Conduct, and the latter of Death. But their relation to one another in the suicide is one which we cannot hope to discover in this problem, though the relation of the curves may perhaps throw some little light upon it.

The study of the effects of the weather conditions upon the physical strength of the students in the gymnasium is at the time of writing not completed, and a report of progress only can be made. It, however, closely resembles the curve for the death rate, in that it shows deficiencies in strength for the *katabolic* condition.

As a conclusion, it would seemingly be safe to say that of the activities (or cessation of activity) possible to human beings, some are the result of excessive vitality, and others of deficient states; and that, generally speaking, those misdemeanors which have been classed under our study as those of Conduct are the results of the former, while death is an accompaniment of the latter.

I must add that I fully recognize that an insufficient number of data have been considered to give my conclusions anything like the validity which should be desired, and also that many possible classes of data have not been treated at all; but I feel justified in offering the study as a modest contribution to the knowledge upon the subject, with the hope of supplying at some future time many of its deficiencies.

TABLES.

The numbers in the following tables are those upon which the curves at the end of this paper are based. They all represent percentages. Those in the line headed NORMAL show the *normal prevalence* or expectancy for the condition above each. Those after the name of each class of data, the *occurrence* for the condition. The line below, in each case, the relation of *occurrence* to *expectancy*. School data, Death and Suicide are referred to a special normal, not given.

OCCURRENCE.

Norm.	Jan. 8.2	Feb. 7.7	Mar. 8.5	Apr. 8.2	May. 8.5	June. 8.2	July. 8.5	Aug. 8.5	Sept. 8.2	Oct. 8.2	Nov. 8.2	Dec. 8.5
School.	8.1 —19	9.6 + 3	9.8 — 7	10.7 ± 0	12.3 +17	132 +32	.8 —20		3.2 —55	12.9 + 8.	10.2 + 7	9.6 + 7
M. As'lt.	6.7 —21	6.2 —19	7.2 —14	7.6 — 7	8.9 + 7	10. +22	10.9 +28	103 +21	9.2 +12	8.2 — 4	7.4 — 9	7.5 —13
F. As'lt.	5.3 —37	5.5 —30	6.4 —24	6.4 —22	9. + 6	11.4 +38	11.5 +38	14.3 +68	10. +22	7.8 — 8	6.5 —21	6.3 —14
Pen.	9.8 +15	7.5 — 3	9.4 +10	9. +10	8.1 — 1	8.2 ± 0	7.5 —12	9.3 + 9	8.3 + 1	7.8 — 9	7.4 —10	7.4 —13
M. Ins.	8.2 — 4	6.3 —18	9.3 + 9	8.1 — 1	9.7 +14	9.5 +15	9.3 + 9	8.9 + 5	8. + 2	7.1 — 6	6.8 —17	8.5 ± 0
F. Ins.	7.3 —14	7.5 — 3	9.5 +12	8.6 + 5	10.4 +22	9.6 +17	8. — 6	8.5 ± 0	9.1 +11	8.5 ± 0	5.4 —34	7.3 —14
Death.	8.5 ± 0	7.7 ± 0	8.7 + 2	8. — 2	7.7 —10	7.9 — 4	11.8 +39	8.6 + 8	6. — 7	7.7 — 9	7.8 — 5	8.9 + 5
Suicide.	5.9 —30	7.3 — 5	7.4 — 3	9.2 +12	10.3 +25	8.7 + 6	12.4 +46	10.4 +23	8. — 1	8.5 ± 0	5.9 —30	7.2 —16
Banks.	7.4 —13	7.4 — 4	8.7 + 2	7.1 —13	7.4 —12	8.7 + 6	10. +17	9.3 +10	7.8 — 5	8.7 + 6	8.9 + 9	8.9 + 5

	CHARACTER OF DAY.			PRECIPITATION.	
Normal.	Clear 33.1.	Pt. Cloudy 36.4	Cloudy 30.4	None 45.8.	+.01 in 54.2.
School.	33.5 — 1	35.8 + 9	31.6 — 6	56.1 + 1	43.9 — 1
Male Ass'lt.	34.7 + 5	38. + 5	27.3 — 9	52.1 +11	47.9 —11
Fem. Ass'lt.	34.2 + 3	40.2 +10	25.6 —12	57.1 +26	42.9 —20
Peniten.	35.1 + 6	3.61 — 1	28.8 — 5	54.5 +21	44.5 —19
Male Ins.	33.1 ± 0	37.3 + 3	29.6 — 2	54.3 +12	45.3 —18
Fem. Ins.	54.3 + 3	36.2 — 1	30. — 1	54.8 +18	55.2 —16
Death.	46.6 + 2	34. — 2	19.3 ± 0	58.4 + 2	41.6 — 2
Suicide.	42.5 + 3	34.5 — 1	22.9 — 4	61.3 + 9	36.9 —14
School At.	— 6	— 8	—10	— 8	—10
Banks.	33.3 — 4	33.7 + 1	33. + 3	52.2 — 2	47.8 + 2

DENVER.

Norma.	Clear. 41.	Pt cloudy. 43.5	Cloudy. 15.5
School.	+ 3	+6	—22
Suicide.	+ 2	—3	+10
Murder.	—14	—2	+3

TEMPERATURE.

NORMAL.	0 5 .1	5 10 .2	10 20 .8	15 20 1.1	20 25 3.1	25 30 55	30 35 7.2	35 40 7.1	40 45 10.	45 50 7.1	50 55 8.9	55 60 7.3	60 65 8.1	65 70 9.9	70 75 11.9	75 80 6.9	80 85 2.2	85 90 .3
School.	.7 +75	.2 ± 0	1.6 ± 0	1.9 —10	4.2 — 2	7.8 —20	9.9 — 6	6.6 —20	8.9 — 7	12.1 +11	13.8 +24	7.5 + 6	7.9 +20	9.5 — 5	6. + 2	2. — 5	.16 — 50	.17 — 50
M. Asl't.	.1 ± 0	.1 — 50	.7 —12	.8 —27	2.3 —25	4. —27	6.5 —10	6.6 — 7	8.4 —16	7.3 + 2	7.8 —12	7.2 — 1	8.7 + 7	10.9 +10	14.8 +24	9.3 +35	3.7 + 68	.4 + 33
F. As'lt.		.2 ± 0	.3 —63	1.2 ± 0	1.8 —42	3.2 —42	5.2 —28	6.4 —10	6.3 —37	5.9 —17	6.4 —28	6.7 — 8	7.6 — 6	11.7 +18	16.2 +36	10.4 +52	4.4 +100	.2 — 33
Peniten.	.1 ± 0	.05 — 3.0	.5 —36	1.3 +20	2.5 —19	6. + 9	8.7 +20	8.4 +18	10.5 + 5	6.5 — 8	8.4 — 6	7.5 + 3	8.3 + 2	9.4 — 9	12. + 1	7.6 + 9	2.7 + 23	.8 +160
M. Ins.		.12 — 40	1.1 +37	1.2 +10	2.4 —22	5.9 + 7	6.6 — 4	8.2 —13	9.2 — 8	7.4 + 4	8.1 —10	7. — 3	9.2 +13	10.6 + 6	12.7 + 7	7. + 2	3.1 + 40	.1 — 66
F. Ins.			1.2 +50	.5 —50	3. — 3	5.3 — 4	7. — 3	7.8 — 3	9.3 — 7	6.3 —11	8.5 — 5	8.3 —14	9.4 +16	10.6 + 7	11.3 + 6	9. +30	3. + 36	.2 — 33
Death.	.1 ± 0	.2 ± 0	1.2 ± 0	1.7 —15	3.9 — 2	4.2 + 2	7.5 — 1	10.8 + 1	8.5 — 1	6.2 — 6	7.4 — 7	7.4 — 7	8.2 — 9	11.8 — 2	12. +14	8.6 +36	.9 + 80	
Suicide.	.1 ± 0	.4 + 33	1.4 +15	1.4 —30	3. —25	3.2 —21	6.5 —14	7.3 —31	7.9 +20	6.3 — 5	7.6 — 5	7.9 — 1	8. —10	13.7 +14	14. +33	9.3 +45	.8 + 60	
Sch. Att.	—15	— 10	—10	—10	— 5	—10	—10	— 8	— 9	— 7	— 7	— 7	—10	—12	—10	—10	— 14	— 8
Banks.			1.1 — 8	1.2 —20	2.5 — 4	6.8 + 4	9.5 ± 0	6.4 —11	6.6 — 6	7.8 — 3	10. — 2	5.8 —20	9.1 + 5	11.5 + 5	13.3 + 6	5.5 + 4	2.3 + 43	.16 + 66

DENVER.

(Min.) Normal.	0 10 4.	10 20 9.	20 30 23.	30 40 21.	40 50 16.	50 60 18	60 70 5.	70 80 1.5	80 90 .5	90 100
School		+100	+16	—20	+15	+16	+20	—60	—300	
Suicide		— 50	+20	—16	—35	+20	+23	+28	+ 50	+ 75
Murder		— 60	—50	—33	+15	—20	—21	+33	+ 38	+100

HUMIDITY.

Normal.	30 35 .04	35 40 .1	40 45 .6	45 50 1.9	50 60 5.	55 60 7.4	60 65 10.7	65 70 14.1	70 75 12.8	75 80 12.8	80 85 12.	85 90 9.4	90 95 7.7	95 100 5.1	100 .4
School.				2.5 − 8	5.6 +33	7.6 + 2	9.4 +12	15.6 + 3	15.1 − 1	12.5 + 3	10.2 ± 0	7.2 + 4	9.1 − 1	12.2 +37	
M. Ass'lt.	.1 +100	.1 ± 0	.5 −10	2.3 +21	5.4 + 8	8.3 +12	11.3 + 6	13.8 − 2	12.9 + 1	12.6 − 2	11.4 − 5	9.9 + 5	7.2 − 7	3.8 −25	
F. Ass'lt.	.07 + 60	.1 ± 0	.2 −66	3. +58	5.5 +10	8.8 +19	11.8 +10	15.1 + 7	14. + 9	14. + 9	12.1 + 1	8.9 − 5	3.2 −58	2.9 −42	
Peniten.		.1 ± 0	.8 +33	2.3 +21	6. +20	7.1 − 4	11. + 3	13.8 − 2	14.3 +12	12.9 + 1	10.4 −13	9.5 + 1	8.4 + 9	3.6 −29	
M. Ins.			.6 ± 0	2.7 +50	4.8 − 4	8.6 +16	10.4 − 3	14.2 + 2	11.3 −12	12. − 7	11.6 − 4	10.7 +14	6.6 −15	39. −23	
F. Ins.			.6 ± 0	2.7 +40	4.3 −14	9.1 +23	12.1 +23	15.3 + 8	12.2 − 5	11.9 − 7	10.7 −11	10.2 + 9	6.4 −17	4. −21	
Death.		.1 ± 0	1.6 + 7	4.4 − 2	7.7 − 2	10.4 + 4	12.6 − 5	12.7 + 6	12.6 − 5	12.6 + 3	8.1 + 3	7.2 + 3	5.6 ± 0	3.2 + 3	
Suicide.		.1 ± 0	1.7 +13	4.1 − 9	8.8 +13	8.8 −12	13. − 2	13.1 + 9	15 +13	11.3 − 3	6.1 −23	8.1 +16	5. −10	3.5 +13	
Sch. Att			− 9	−10	−11	−11	− 8	− 7	− 8	− 9	− 6	− 9	−10	−13	
Banks.		.3 +80	.4 +43	1.1 − 8	3.3 −19	6.4 − 7	9.6 − 2	11.1 − 4	12.2 − 6	13.1 + 2	13.6 + 5	9.4 + 1	10. + 8	9.3 +10	

DENVER.

(Min.) Normal.	10 20 4.	20 30 17.	30 40 23.	40 50 21.	50 60 12.	60 70 11.	70 80 5.	80 90 4.5	90 100 3.
School.	+500	+700	+ 50	+ 11	+10	−15	−16	± 0	−33
Suicide.	+ 40	+800	+110	+ 90	+12	−18	−39	− 7	−40
Murder.	+400	+300	+125	+100	−14	−33	−20	−46	− 9

BAROMETER.

Norm.	29 00 10 .3	10 20 .5	20 30 .7	30 40 16	40 50 3.5	50 60 6.7	60 70 13.7	70 80 19.3	80 90 19.	90 30.00 15 1	30 00 10 8.9	10 20 5 9	20 30 2.8	30 40 1.4	40 50 5.	50 60 .3
School.	.9 ± 0	.8 +14	1.9 +12	4.7 +11	7.9 + 7	12.1 + 5	15.3 + 7	17.5 ± 0	19.3 +13	10.9 — 4	4. —38	3.5 — 3	.8 —20	— .7 +20		
M. As'lt.	.4 +33	.4 ± 0	.7 ± 0	2.2 +37	4.8 +37	8.8 +31	16.4 +18	19.0 — 2	18.8 — 2	13.8 — 8	6.7 —22	4.7 —20	19 —32	.9 —35	.3 —66	.13 —56
F. As'lt.	.06 —80	.3 —40	.6 —15	1.7 + 6	5.6 + 60	8.4 +25	16.3 +19	208 + 8	18. — 5	13.1 —13	7.1 —20	4.9 —17	1.8 —35	.7 —15	.4 —20	.1 —66
Pen.	.5 +66	.7 +40	.7 ± 0	2.8 +75	52 + 49	9.8 +46	13.8 + 1	18.7 — 4	17.5 — 7	13.7 — 9	7.2 —19	4.8 —18	2.4 —14	1.2 —14	.3 —40	
M. Ins.	.5 +66	.5 ± 0	.6 —15	2.8 +43	4.7 +34	7.9 +18	14. + 2	20. + 4	15.7 —17	15.2 + 1	7.9 —11	5.3 —10	24 —14	.9 —36	.4 —20	.15 —50
F. Ins.	.5 +66	.5 ± 0	.6 —12	2.5 +56	5.6 +60	8.7 +30	15.9 +16	18.7 — 3	17.6 — 7	14.5 — 4	6.5 —27	5.7 — 4	1.5 —46	1.3 — 7	.2 —60	
Sch.Att.	—12	—12	—12	—12	—13	—88	— 9	— 7	— 7	— 6	— 8	—10	—11	— 7		
Banks.	.2 ± 0	.7 +40	1.2 ± 0	4 4 — 2	8.6 —11	12.2 — 5	20. + 5	18.3 +14	14. ± 0	10.4 + 6	4.5 ± 0	3.9 — 4	.5 ± 0			

DENVER.

Normal.	24.20 30 .5	24.30 40 2.5	40 50 5.5	50 60 11.5	60 70 15.5	70 80 23.5	80 90 20.0	90 25.00 14.5	25.10 5.	25.10 20 1.5
School.	+100	+ 90	+ 80	+50	—23	—24	—24	— 6	+ 25	
Suicide.	+150	+150	+50	+10	—20	— 3	—10	— 3	—150	
Murder.	—50	+ 20	+33	± 0	—13	+18	—18	—20	± 0	

WIND.

Norm.	0 50	50 100 .9	100 150 10.5	150 200 19.2	200 250 21.8	250 300 15.2	300 350 10.9	350 400 7.7	400 450 5.8	450 500 2.6	500 550 1.6	550 600 1.3	600 650 .5	650 700 .4	700 750 .2
School.		.1 —50	4.6 —18	10.8 — 2	16.8 — 3	16.6 — 2	13.2 — 2	10.3 + 4	9 + 3	5.2 +13	4.2 — 2	1.5 —21	1.1 —18	.9 ± 0	.5 —28
M. As'lt.		.8 —11	10.8 — 3	21.3 +10	22.6 + 4	15.6 + 3	10.3 — 5	7.3 — 6	5.3 — 8	2.5 — 4	1.5 — 6	1.2 — 8	.5 ± 0	.4 ± 0	.15 —25
F. As'lt.		.4 —55	12.1 +15	21.7 + 7	21.6 —13	14.1 — 7	10. — 8	6.2 —19	5.5 — 5	2.3 —11	1.1 —32	.7 —40	.5 ± 0	.2 —25	.12 —75
Pen.		.7 —20	10.6 + 1	19.9 + 3	22.1 + 1	15.1 — 1	10.8 — 1	7.5 — 3	6.5 —12	3.6 +38	1.6 ± 0	1.3 ± 0	.3 —40	.3 —25	.2 ± 0
M. Ins.		.6 —33	10.6 + 1	20 + 5	22.3 + 2	14.5 — 5	11.7 + 8	8.3 + 8	5.3 —10	2.9 +11	1.3 —20	1.1 —15	.5 ± 0	.5 +25	.2 ± 0
F. Ins.		.3 —66	10.8 + 3	19.5 + 2	21.7 ± 0	15.1 — 1	10.7 — 2	8.6 +12	7.3 +26	2. —23	1.6 ± 0	1.2 — 8	.6 +20	.5 +28	
Death.		10.9 + 4	24.4 + 2	22.2 + 2	17.7 — 4	7.9 + 1	5.9 — 6	4. + 5	2.8 — 4	1.5 ± 0	1.5 — 6	.3 ± 0	.6 +20	.3 ± 0	.5 +25
Suicide.		12.6 +20	22.9 — 4	25.9 +18	13.3 —18	7.3 — 6	5.3 —16	5.3 +39	3.3 —14	1.1 —27	.1 —38	.3 ± 0	.3 —40	.5 +66	.4 ± 0
Sch. Att.		—29	— 9	— 9	— 9	— 9	— 8	— 9	— 6	— 8	— 8	— 8	—13	— 8	—15
Banks.		.3 ± 0	3.4 + 3	13. + 8	16.3 + 3	18.3 ± 0	15.2 — 2	10.5 — 3	7.4 — 3	4.2 — 8	2.6 —28	1.9 —10	2.4 — 8	.6 —40	.6 ± 0

DENVER.

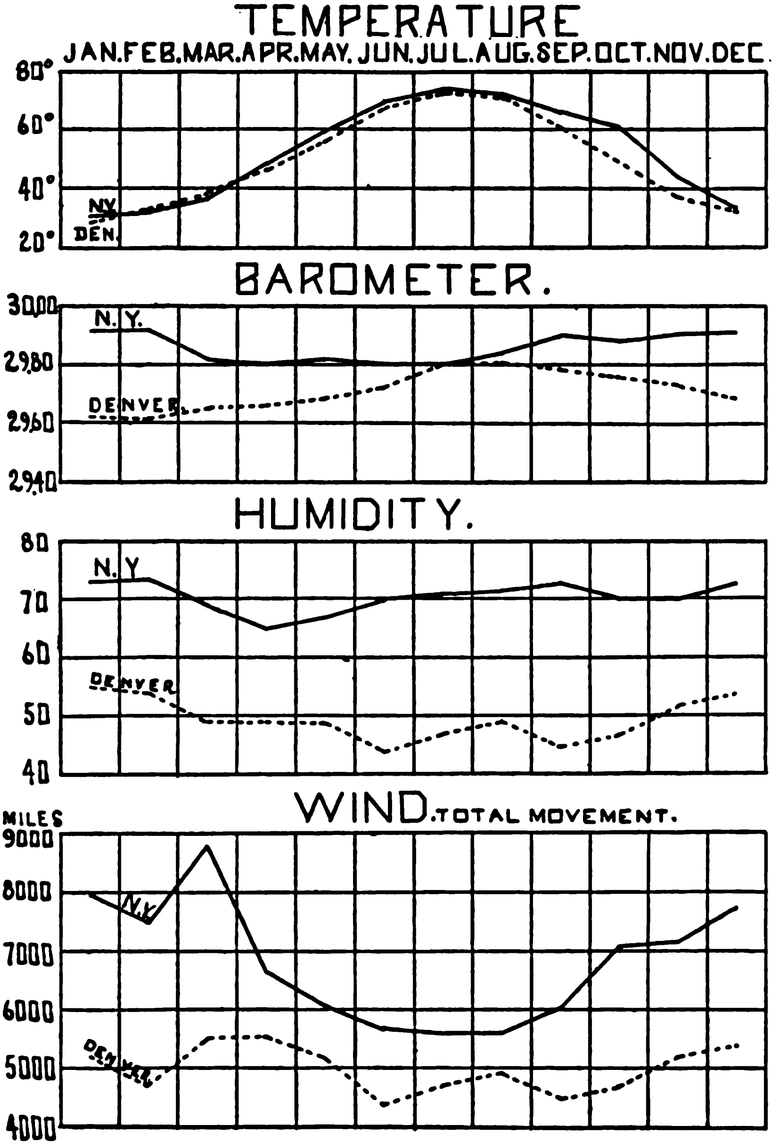
Normal.	50 100 6.5	100 150 48.5	150 200 29.	200 250 11.	250 300 9.5	300 350 .8	350 400 .3
School.	—66	— 4	+ 6	+10	+ 28	+200	+450
Suicide.	—85	—25	+25	+21	+250	+300	+400
Murder.	—55	—15	± 0	± 0	+240	+400	+400

DESCRIPTION OF FIGURES.

In the figures the meteorological conditions are indicated at the top, and in any column the readings are for the condition at its head. Percentages of *excess* or *deficiency* are read in horizontal distances from the base line to which each curve is referred. Distances above the base line indicate *excess;* below, *deficiency* in the occurrence of data with which the curve deals. The lighter, horizontal lines show differences of 20%. In the curves for Denver, on the Temperature, Barometer and Humidity charts, meteorological groups are used, equal to two of those given at the top. For that city, too, five inches must be subtracted from the readings on the Barometer chart. For a fuller description of the curves see page 12.

FIG. 8.

MONTHLY MEANS



TEMPERATURE

BAROMETER.

HUMIDITY.

WIND. TOTAL MOVEMENT.

FIG. 9

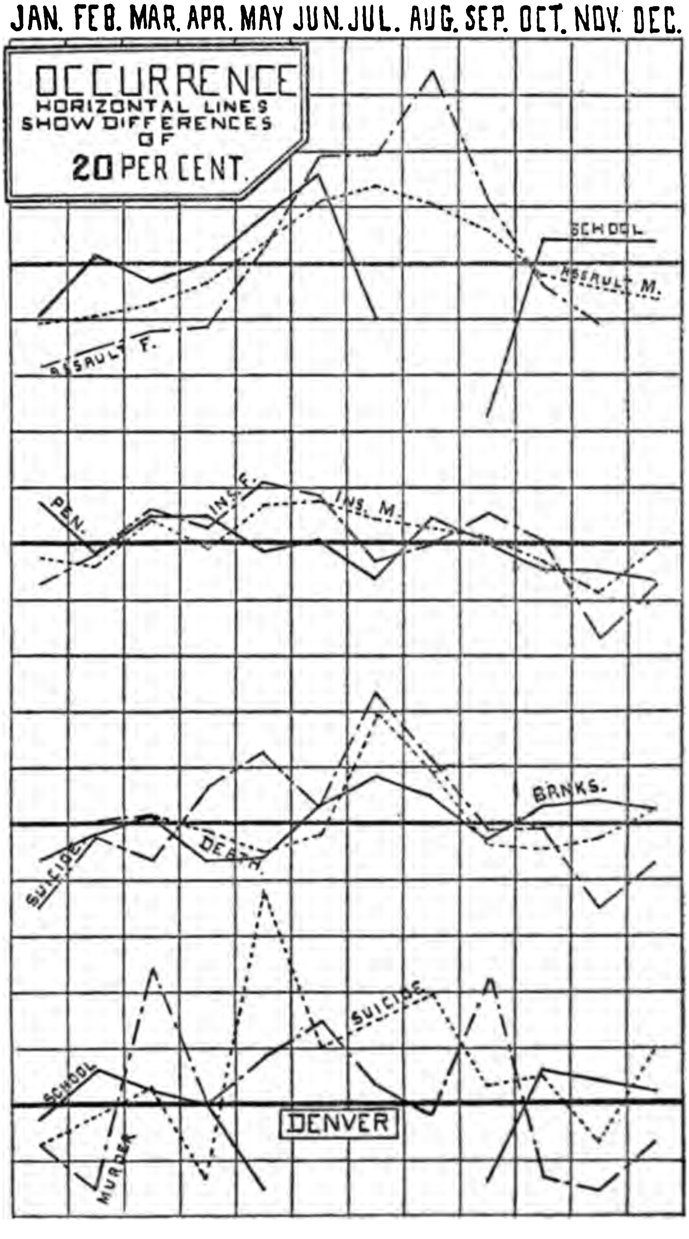

JAN. FEB. MAR. APR. MAY JUN. JUL. AUG. SEP. OCT. NOV. DEC.
OCCURRENCE
HORIZONTAL LINES SHOW DIFFERENCES OF 20 PER CENT.
SCHOOL
ASSAULT M.
ASSAULT F.
PEN.
INS. F.
INS. M.
BANKS.
SUICIDE
DEATH
SUICIDE
SCHOOL
MURDER
DENVER

FIG. 10

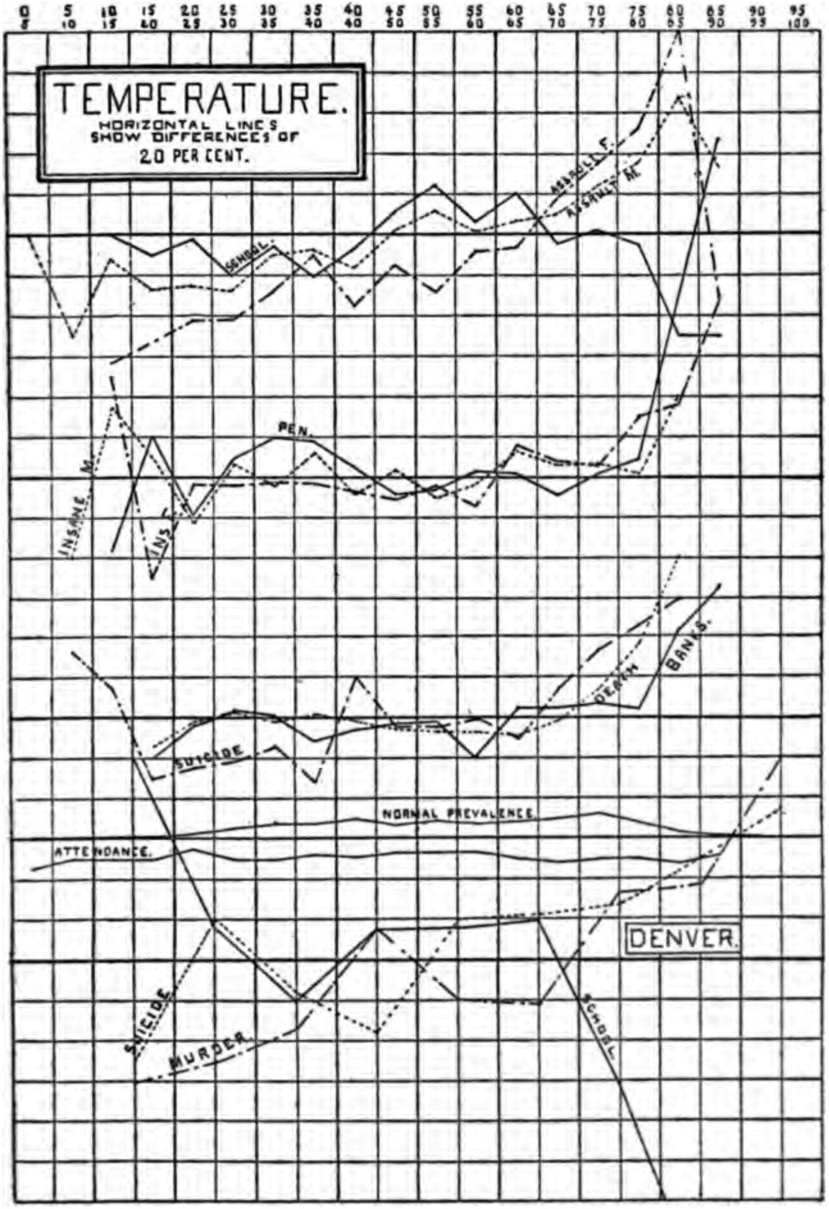

0 5
5 10
10 15
15 20
20 25
25 30
30 35
35 40
40 45
45 50
50 55
55 60
60 65
65 70
70 75
75 80
80 85
85 90
90 95
95 100
TEMPERATURE.
HORIZONTAL LINES SHOW DIFFERENCES OF 20 PER CENT.
ASSAULT F.
ASSAULT M.
SCHOOL.
PEN.
INSANE M.
INS. F.
SUICIDE.
DEATH
BANKS.
NORMAL PREVALENCE.
ATTENDANCE.
DENVER.
SUICIDE.
MURDER.
SCHOOL.

FIG 11.

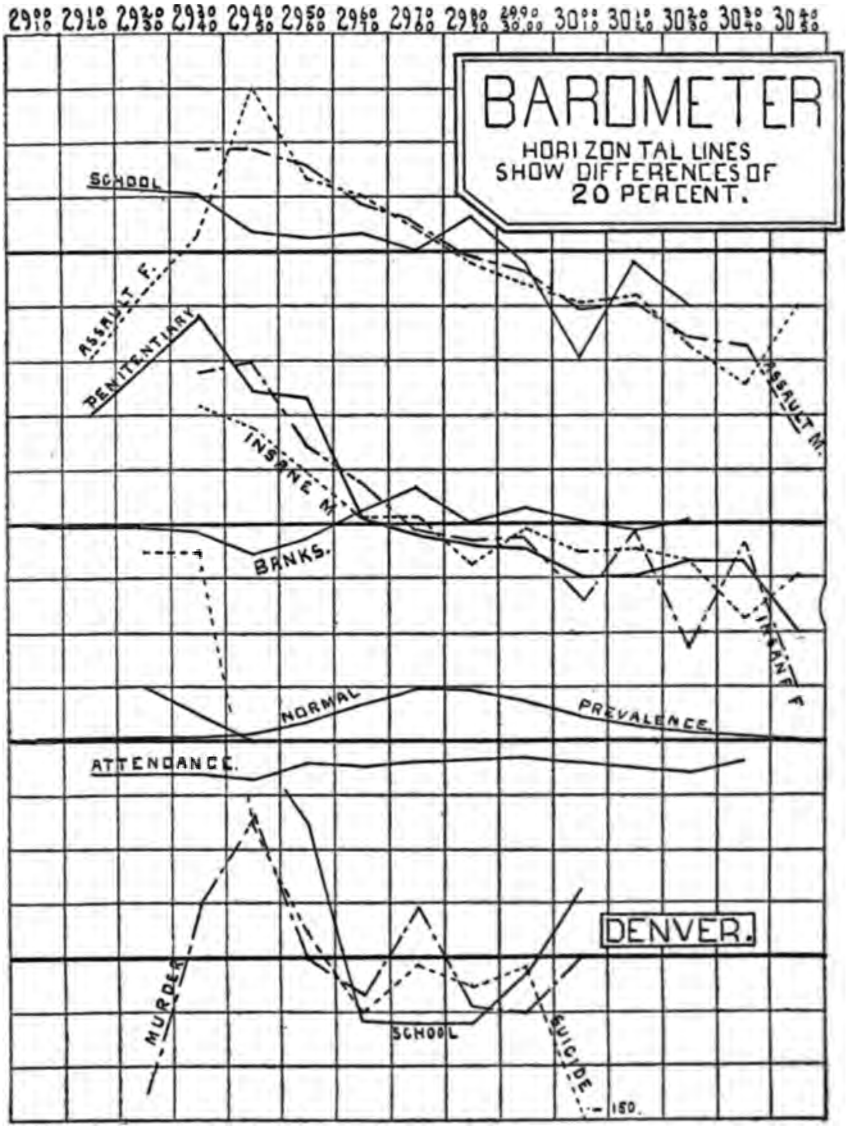

29 .00 .10
29 .10 .20
29 .20 .30
29 .30 .40
29 .40 .50
29 .50 .60
29 .60 .70
29 .70 .80
29 .80 .90
29.90 30.00
30 .00 .10
30 .10 .20
30 .20 .30
30 .30 .40
30 .40 .50
BAROMETER
HORIZONTAL LINES SHOW DIFFERENCES OF 20 PERCENT.
SCHOOL
ASSAULT F.
PENITENTIARY
ASSAULT M.
INSANE M.
BANKS.
INSANE F.
NORMAL
PREVALENCE.
ATTENDANCE.
DENVER.
MURDER
SCHOOL
SUICIDE
150.

FIG. 12

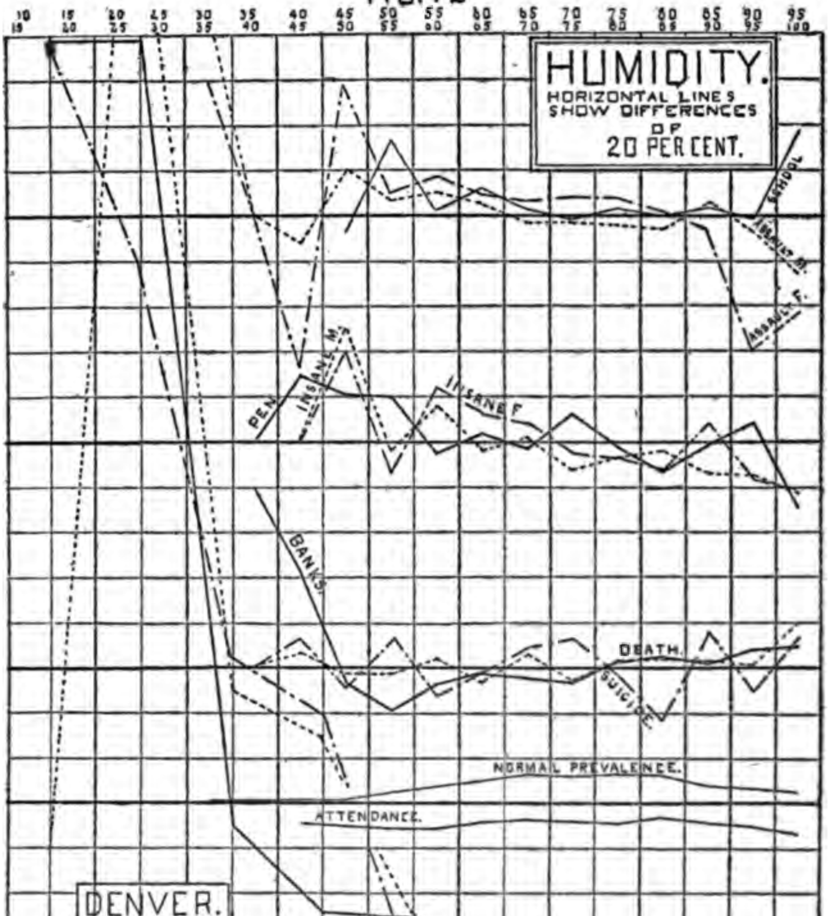

10 15
15 20
20 25
25 30
30 35
35 40
40 45
45 50
50 55
55 60
60 65
65 70
70 75
75 80
80 85
85 90
90 95
95 100
HUMIDITY.
HORIZONTAL LINES SHOW DIFFERENCES OF 20 PER CENT.
SCHOOL
ASSAULT M.
ASSAULT F.
PEN.
INSANE M.
INSANE F.
BANKS.
DEATH.
SUICIDE.
NORMAL PREVALENCE.
ATTENDANCE.
DENVER.
SCHOOL.
SUICIDE.
MURDER.

FIG. 13.

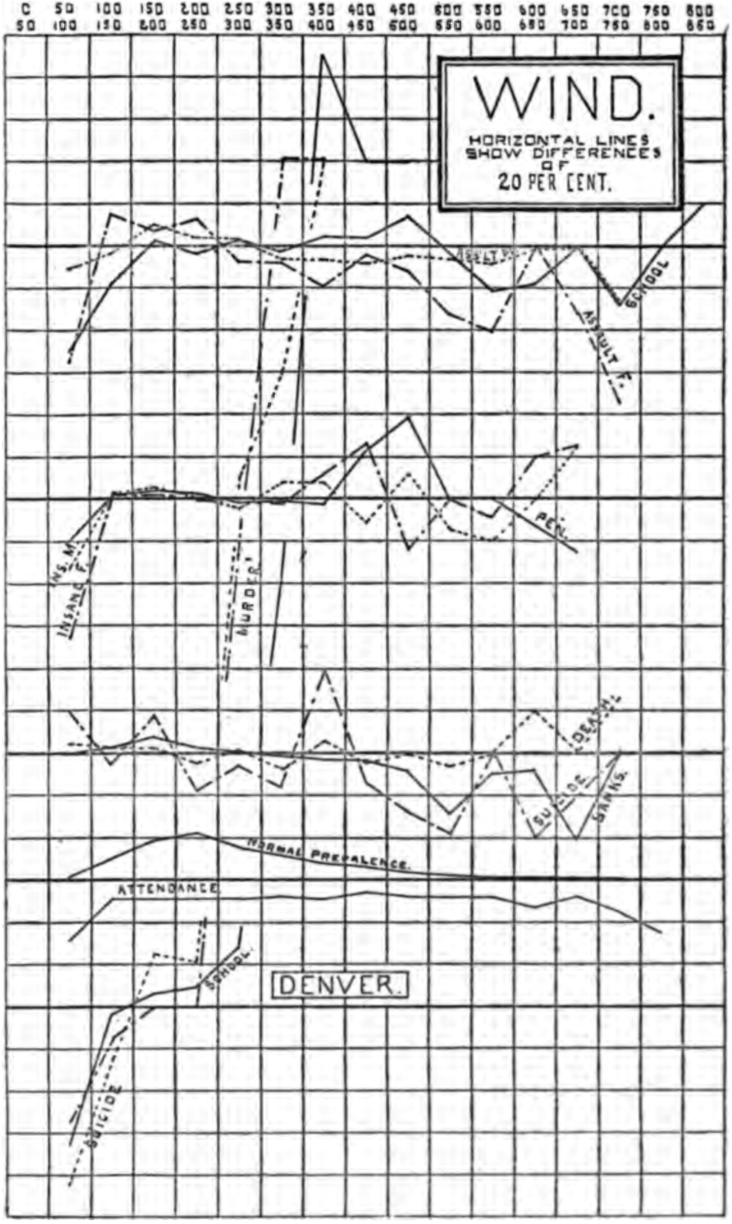
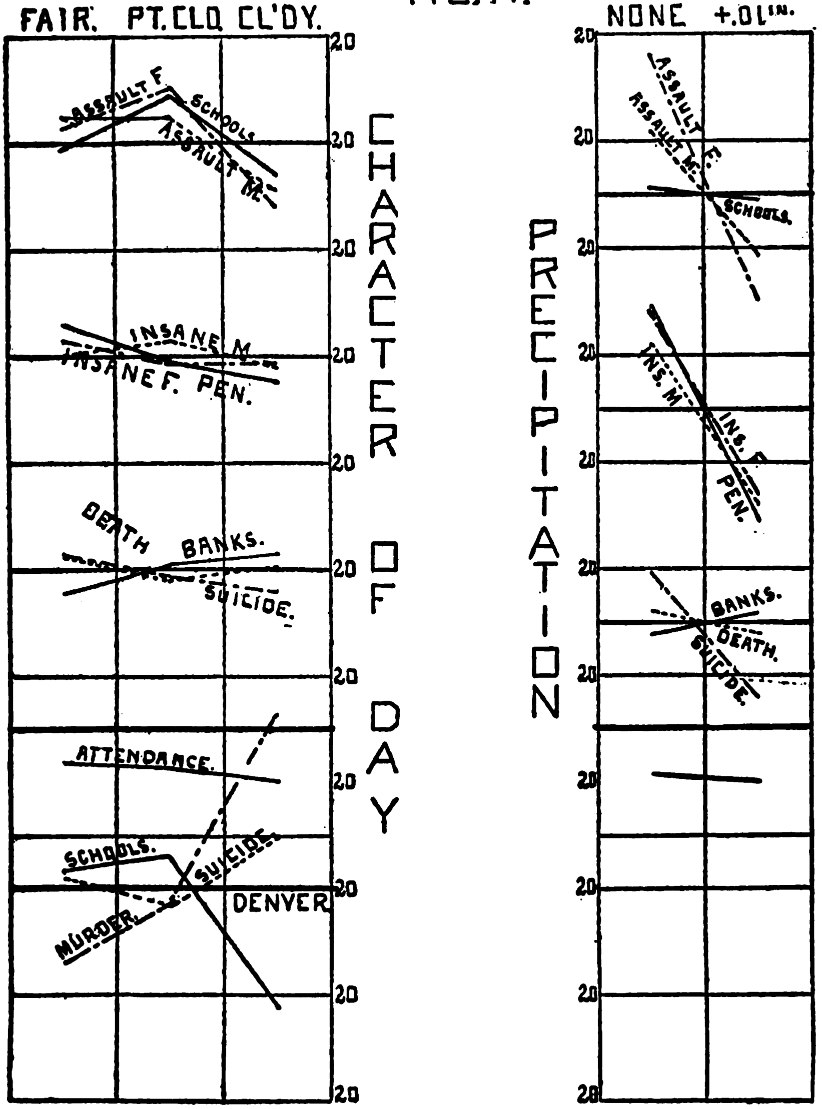

C 50 100 150 200 250 300 350 400 450 500 550 600 650 700 750 800
50 100 150 200 250 300 350 400 450 500 550 600 650 700 750 800 850
WIND.
HORIZONTAL LINES SHOW DIFFERENCES OF 20 PER CENT.
ASSLT. M.
SCHOOL
ASSAULT F.
INS. M.
INSANE F.
MURDER
PEN.
DEATHS.
SUICIDE.
ARRS.
NORMAL PREVALENCE.
ATTENDANCE
SCHOOL.
SUICIDE
DENVER.

FIG. 14.


FAIR.
PT.CLD.
CL'DY.
20
CHARACTER OF DAY
ASSAULT F.
SCHOOLS
ASSAULT M.
INSANE M.
INSANE F.
PEN.
DEATH
BANKS.
SUICIDE.
ATTENDANCE.
SCHOOLS.
SUICIDE.
MURDER.
DENVER
NONE
+.01 IN.
PRECIPITATION
ASSAULT F.
ASSAULT M.
SCHOOLS.
INS. M.
INS. F.
PEN.
BANKS.
DEATH.
SUICIDE.

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